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## The Foucault Pendulum Star Path and the $n$-Leaved Rose


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IT is well known that a body acted upon by a force proportional to the displacement and of opposite sign will undergo simple harmonic motion. If the body is given an initial velocity at right angles to its initial displacement from the center of force, its subsequent motion will be in a plane elliptic path. This path is approximately that of the Foucault pendulum, and it is called the *Chevilliet ellipse* when it is referred to coordinates fixed in space, in which the earth rotates with its component of angular velocity at the latitude of the point of support of the pendulum. The motion with respect to the fixed coordinates is elliptic rather than rectilinear because, although the pendulum bob is started from a rest position on the earth, a slight motion at right angles to its initial displacement is imparted by the rotation of the earth.

The present treatment of the Foucault pendulum path claims only the degree of accuracy of this plane motion on the earth's surface under a central force that would be rectilinear or elliptic if referred to fixed coordinates. The departures from these paths are taken as entirely caused by the spinning of the plane of reference about an axis normal to the earth's surface with angular velocity equal to the earth's component of spin at the latitude in question.

When this motion is referred to coordinates fixed on the rotating earth, the result is the familiar Foucault pendulum path, which—we here note—is associated with an $n$-pointed star, $n$ being the ratio of the earth's period of rotation to the natural period of the pendulum. This is also the ratio of the major to the minor axis of the Chevilliet ellipse. If $n$ is an odd integer, the path is exactly an $n$-pointed star of the type whose sides have outward curvature, the points being cusps. If $n$ is an even integer, the path is a $2n$-pointed star, since each half or one way swing will then generate a different star point or cusp. If the ratio $n$ is not integral, the path does not repeat itself in successive periods of the earth's rotation and hence is not a true star. Since, in the actual case, $n$ is of the order of $10^4$, the motion during one-half swing is almost rectilinear.

If an initial velocity is imparted at right angles to the initial displacement of the pendulum bob, equal in magnitude but opposite in direction to the surface velocity due to rotation—in middle latitudes about 6 in./hr—the motion becomes linear simple harmonic motion in coordinates fixed in space and produces the familiar $n$-leaved rose with respect to coordinates rotating with the earth's component of spin. This path, in contrast with the star path, passes through the center of force; but, like the other path, it has $n$ leaves (instead of points) when $n$ is an odd integer, $2n$ when $n$ is an even integer and an undetermined number when $n$ is not an integer.

Since the initial shove at right angles to the initial displacement of the pendulum bob is an effect so minute that it may be accidental, the $n$-leaved rose is a not unlikely actual path for

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the Foucault pendulum bob. Since for either the rose path or the star path, the normal path deflection per swing is of the order of 1 part in 10,000, the two paths are indistinguishable to the ordinary observer, and may be equally considered as the actual Foucault pendulum path to the order of exactness of the present treatment.

### The Theorem of Chevilliet

We treat the simplified case where the motion is in two dimensions in a horizontal plane on the surface of the earth, under the action of a central force that is proportional in magnitude but opposite in sign to the displacement from the origin; the latter is taken under the point of suspension of the pendulum bob.

The classical equations of motion of the pendulum bob are

$$\begin{aligned}\frac{d^2x}{dt^2}-2\omega_L\frac{dy}{dt}&=-\frac{g}{l}x=-\omega_P{}^2x,\\ \frac{d^2y}{dt^2}+2\omega_L\frac{dx}{dt}&=-\frac{g}{l}y=-\omega_P{}^2y,\end{aligned}\tag{1}$$

where $\omega_L=\omega\sin L$ is the component of the earth's rotation at latitude $L$ and $\omega_P[=(g/l)^{\frac{1}{2}}]$ will be recognized as the angular velocity in the reference circle whose projection on its diameter is the simple harmonic motion of the simple pendulum that would occur for small displacements in the absence of the Coriolis terms $2\omega_Lv$. The natural period of the motion is

$$T=2\pi/\omega_P=2\pi(l/g)^{\frac{1}{2}}.\tag{2}$$

The theory underlying this classical treatment is that the bob swings, under the action of a force that is proportional to the displacement, in an elliptical path (Chevilliet's theorem) from which it departs solely in consequence of the rotating coordinate system employed in Eqs. (1) and used to refer the motion to axes fixed on the rotating earth. By introducing the complex variable $u=x+iy$, Eqs. (1) may be combined to give

$$\frac{d^2u}{dt^2}+2i\omega_L\frac{du}{dt}+\omega_P{}^2u=0.\tag{3}$$

This is a linear differential equation with constant coefficients, whose characteristic equation is

$$m^2+2i\omega_Lm+\omega_P{}^2=0;$$

it has pure imaginary roots $i\alpha$ and $-i\beta$, where

$$\alpha=(\omega_P{}^2+\omega_L{}^2)^{\frac{1}{2}}-\omega_L,\quad \beta=(\omega_P{}^2+\omega_L{}^2)^{\frac{1}{2}}+\omega_L.\tag{4}$$

The solution of Eq. (3) is therefore

$$u=Ae^{i\alpha t}+Be^{-i\beta t},$$

and

$$\frac{du}{dt}=iA\alpha e^{i\alpha t}-iB\beta e^{-i\beta t}.\tag{5}$$

If the bob starts from rest at $(b, 0)$ in the $x, y$ plane, we find, by setting $t=0$ in Eq. (5),

$$b=A+B\quad\text{and}\quad A\alpha=B\beta;$$

hence

$$A=b\beta/(\alpha+\beta),\quad B=b\alpha/(\alpha+\beta),$$

and

$$u=\frac{b}{(\alpha+\beta)}(\beta e^{i\alpha t}+\alpha e^{-i\beta t})=x+iy.\tag{6}$$

Separating the real and imaginary parts, we have the parametric equations of the path,

$$\begin{aligned}x&=\frac{b}{\alpha+\beta}(\beta\cos\alpha t+\alpha\cos\beta t),\\ y&=\frac{b}{\alpha+\beta}(\beta\sin\alpha t-\alpha\sin\beta t).\end{aligned}\tag{7}$$

On squaring and adding Eqs. (7), we obtain

$$r^2=x^2+y^2=\frac{b^2}{(\alpha+\beta)^2}[\alpha^2+\beta^2+2\alpha\beta\cos(\alpha+\beta)t].\tag{8}$$

The path given by Eqs. (7) may be referred to axes fixed in space (indicated by primed variables) and hence rotating with respect to the earth, by means of the equations

$$\begin{aligned}x'&=x\cos\omega_Lt-y\sin\omega_Lt,\\ y'&=x\sin\omega_Lt+y\cos\omega_Lt.\end{aligned}\tag{9}$$

When Eqs. (7) are substituted in Eqs. (9), we have

$$\begin{aligned}x'&=\frac{b}{\alpha+\beta}[\beta\cos(\alpha+\omega_L)t+\alpha\cos(\beta-\omega_L)t]\\ &=b\cos(\omega_P{}^2+\omega_L{}^2)^{\frac{1}{2}}t,\\ y'&=\frac{b}{\alpha+\beta}[\beta\sin(\alpha+\omega_L)t-\alpha\sin(\beta-\omega_L)t]\\ &=b\frac{\beta-\alpha}{\beta+\alpha}\sin(\omega_P{}^2+\omega_L{}^2)^{\frac{1}{2}}t,\end{aligned}\tag{10}$$

where the simplification comes from introducing Eqs. (4), which show that

$$(\omega_P^2+\omega_L^2)^{\frac{1}{2}}=\alpha+\omega_L=\beta-\omega_L,$$

or

$$\beta-\alpha=2\omega_L \quad \text{and} \quad \beta+\alpha=2(\omega_P^2+\omega_L^2)^{\frac{1}{2}}. \tag{4a}$$

Equations (10) are seen to be the equations of an ellipse, which in view of Eqs. (4a) can be written

$$\frac{x'^2}{b^2}+\frac{y'^2}{b^2}\left(\frac{\omega_P^2+\omega_L^2}{\omega_L^2}\right)=1; \tag{11}$$

this is the equation of the ellipse in standard form, referred to coordinates fixed in space with the earth spinning beneath.

The constant $\omega_P$, the angular velocity in the reference circle of the pendulum whose natural period $T$ is given by Eq. (2), is about $10^4\omega_L$, the earth's component of angular velocity; hence $\omega_L^2$ can be neglected in comparison with $\omega_P^2$, and Eq. (11) becomes

$$\frac{x'^2}{b^2}+\frac{y'^2}{b^2}\frac{\omega_P^2}{\omega_L^2}=1, \quad \text{or} \quad \frac{x'^2}{b^2}+\frac{y'^2}{b^2}\frac{T_L^2}{T^2}=1. \tag{12}$$

Equations (12) demonstrate Chevilliet's theorem, which states that the path of the Foucault pendulum bob, when referred to fixed axes not rotating with the earth, is an ellipse having major and minor axes proportional to the period[1] of the earth and the natural period of the pendulum.

## The Cusps and Constant Change of Direction of the Path

The velocity components of the pendulum, obtained by differentiating Eqs. (7), are

$$\begin{aligned}\frac{dx}{dt}&=-\frac{b\alpha\beta}{\alpha+\beta}(\sin\alpha t+\sin\beta t),\\ \frac{dy}{dt}&=\frac{b\alpha\beta}{\alpha+\beta}(\cos\alpha t-\cos\beta t).\end{aligned} \tag{13}$$

These are each seen to be zero when $\alpha t=2k\pi-\beta t$, where $k$ is an integer; that is, when

$$t=2k\pi/(\alpha+\beta)=k\pi/(\omega_P^2+\omega_L^2)^{\frac{1}{2}}. \tag{14}$$

The corresponding points are cusps[2]—end points of the swing, where the bob comes to instantaneous rest—whose coordinates are obtained by substituting in Eqs. (7) the values of $t$ given by Eq. (14). When $t=T_L=2\pi/\omega_L$, the earth's rotation period, the bob will have returned to its original position, provided that

$$k(T_L)/2=(\omega_P^2+\omega_L^2)^{\frac{1}{2}}/\omega_L=n; \tag{15}$$

here $n$, an integer, is seen to be the number of complete two-way swings the pendulum bob makes in one complete circuit owing to the earth's rotation.

By dividing the second of Eqs. (13) by the first and introducing Eqs. (4), we obtain for the slope,

$$\frac{dy}{dx}=-\frac{\cos\alpha t-\cos\beta t}{\sin\alpha t+\sin\beta t}=-\tan\omega_L t. \tag{16}$$

Equation (16) shows that the angle of inclination of the path is $-\omega_L t$ or $\pi-\omega_L t$, indicating that the path changes its direction at a constant rate equal to $-\omega_L$, as is to be expected.

If we introduce $n$ from Eq. (15) into the accurate form of the Chevilliet ellipse equation given by Eq. (11), we have it in standard form,

$$\frac{x'^2}{b^2}+\frac{y'^2}{b^2/n^2}=1, \tag{17}$$

which shows that the axes of the ellipse are proportional to the number of swings in a complete circuit. Introducing the identity, $\cos(\alpha+\beta)t\equiv 2\cos^2\frac{1}{2}(\alpha+\beta)t-1$, we get from Eq. (8),

$$r^2=\frac{b^2}{(\alpha+\beta)^2}[(\alpha-\beta)^2+4\alpha\beta\cos^2\tfrac{1}{2}(\alpha+\beta)t]. \tag{18}$$

Or since, by Eqs. (4), $\alpha\beta=\omega_P^2$ and, by Eq. (15), $(\omega_P^2+\omega_L^2)^{\frac{1}{2}}=n\omega_L$, we obtain by introducing Eq. (4a),

$$\begin{aligned}r^2&=\frac{b^2}{\omega_P^2+\omega_L^2}(\omega_L^2+\omega_P^2\cos^2(\omega_P^2+\omega_L^2)^{\frac{1}{2}}t)\\ &=\frac{b^2}{\omega_P^2+\omega_L^2}(\omega_L^2+\omega_P^2\cos^2 n\omega_L t),\end{aligned} \tag{19}$$

[1] A pendulum 25 times the length of a seconds pendulum ($l=85$ ft, approximately) will have a half period of 5 sec, or $T=10$ sec$=1/360$ hr. The period $T_L$ of the earth's rotation in middle latitudes (about 41°49′) is 24/sin 41°49′, or 36 hr. Thus $T_L/T=\omega_P/\omega_L=36\times 360=12{,}960$. In Foucault's original experiment at the Pantheon in Paris, $l$ was 67 m (nearly 3 times 85 ft), so that his $T$ was nearly $10\sqrt{3}$ sec; actually, $T$ was 16 sec and $T_L$ was 32 hr for the higher Paris latitude, so that $T_L/T=\omega_P/\omega_L=7200$.

[2] They are the points of the star path shown in Fig. 2, where, during the local period of the earth's rotation, the bob comes to rest $k(T_L)$ times according to Eqs. (13), (14) and (15), this $k(T_L)$ being also the number of one way swings in a complete circuit.

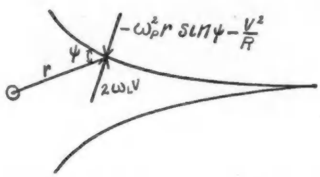


FIG. 1. The Coriolis force balances the centrifugal force plus the normal component of central force.

which shows that the path lies between the circles of radius $b$ and $b/n$, these radii being the semiaxes of the Chevilliet ellipse, the path in nonrotating coordinates given by Eq. (11) or Eq. (17). Equation (19) is the equation of the Foucault pendulum star path (Fig. 2). When $n$ is odd, the path is an $n$-pointed star; when $n$ is even, it is a $2n$-pointed star; and when $n$ is not integral, it has an undetermined number of points.

### Foucault Pendulum Star Path in Polar Coordinates

By making use of the facts that the sum of the force components normal to the path is zero and that the energy of the pendulum is constant, we may set up the differential equation of the Foucault pendulum in polar coordinates. The pendulum bob will be drawn to the origin in the plane and under the point of suspension by the force $f=-\omega_P^2 r$, and the force constant $\omega_P^2$ is related to the natural period of the pendulum according to Eq. (2). Its component normal to the path is $-\omega_P^2 r\sin\psi$, where $\psi$ is the angle between the radius vector and the path, shown in Fig. 1. When this force together with the centrifugal force is balanced against the Coriolis force, we have

$$v^2/R+\omega_P^2 r\sin\psi=2\omega_L v, \tag{20}$$

where $R$ is the radius of curvature of the path, and $v$ is the linear speed of the bob.

Since the normal centrifugal and Coriolis forces do not affect the velocity along the path, the latter is determined entirely by the integral of the central force $f$ along the path. Thus we have

$$\tfrac{1}{2}v^2=-\int \omega_P^2 r\cos\psi \mathrm{d}s=-\int \omega_P^2 r\frac{\mathrm{d}r}{\mathrm{d}s}\mathrm{d}s$$

$$=-\int_b^r \omega_P^2 r\mathrm{d}r,$$

whence the sum of the kinetic and potential energies is proportional to

$$\tfrac{1}{2}v^2+\tfrac{1}{2}\omega_P^2 r^2=\tfrac{1}{2}\omega_P^2 b^2,$$

which is constant; therefore,

$$v=\mathrm{d}s/\mathrm{d}t=\omega_P(b^2-r^2)^{\frac{1}{2}}. \tag{21}$$

The curvature[3] in polar coordinates $(r,\ \theta)$ is given by

$$\frac{1}{R}=-\frac{1}{r}\frac{\mathrm{d}}{\mathrm{d}r}(r\sin\psi)=-\frac{\mathrm{d}\phi}{\mathrm{d}s}, \tag{22}$$

where $\phi=\theta+\psi$, and $\psi=\tan^{-1}(r\mathrm{d}\theta/\mathrm{d}r)$. Then Eq. (20) becomes

$$\frac{\omega_P^2(b^2-r^2)}{r}\frac{\mathrm{d}}{\mathrm{d}r}(r\sin\psi)-\omega_P^2 r\sin\psi$$
$$=-2\omega_L\omega_P(b^2-r^2)^{\frac{1}{2}}.$$

If we divide both members of this equation by the integrating factor $\omega_P(b^2-r^2)^{\frac{1}{2}}/r$, we have

$$\omega_P(b^2-r^2)^{\frac{1}{2}}\frac{\mathrm{d}}{\mathrm{d}r}(r\sin\psi)-\frac{\omega_P r^2\sin\psi}{(b^2-r^2)^{\frac{1}{2}}}=-2\omega_L r. \tag{23}$$

Integration of Eq. (23) gives

$$\omega_P(b^2-r^2)^{\frac{1}{2}}r\sin\psi=\omega_L(b^2-r^2),$$

or

$$\omega_P r\sin\psi=\omega_L(b^2-r^2)^{\frac{1}{2}}, \tag{24}$$

where the constant[4] of integration $\omega_L b^2$ is given by the condition that $\psi$ of Eq. (22) is zero at $r=b$; that is, we have a cusp at this point.

Remembering that $\sin\psi=r\mathrm{d}\theta/(\mathrm{d}r^2+r^2\mathrm{d}\theta^2)^{\frac{1}{2}}$, we may obtain from Eq. (24) an expression for $\tan\psi$ in terms of $r$, and introduce an auxiliary variable,

$$u=\tan\psi=\frac{r\mathrm{d}\theta}{\mathrm{d}r}=\omega_L\left[\frac{b^2-r^2}{(\omega_P^2+\omega_L^2)r^2-\omega_L^2 b^2}\right]^{\frac{1}{2}}$$

$$=\left(\frac{b^2-r^2}{n^2r^2-b^2}\right)^{\frac{1}{2}},$$

where the simplification on the right is obtained by introducing Eq. (15). Solving for $r^2$, we have

$$r^2=b^2(1+u^2)/(1+n^2u^2);$$

then

$$\ln r^2=\ln b^2+\ln(1+u^2)-\ln(1+n^2u^2),$$

[3] Carry out the differentiation of Eq. (22) to get the usual curvature formula in polar coordinates. Each member of Eq. (22) is positive [see Eq. (24)].

[4] This is the constant of areas for motion in the Chevilliet ellipse.

whence

$$dr/r = u\,du/(1+u^2) - n^2u\,du/(1+n^2u^2).$$

Inserting this value of $dr/r$ in the foregoing definition of $u$, we obtain

$$d\theta = u\frac{dr}{r} = \frac{u^2du}{1+u^2} - \frac{n^2u^2du}{1+n^2u^2}$$

$$\equiv \frac{du}{1+n^2u^2} - \frac{du}{1+u^2} = \frac{du}{1+n^2u^2} - d\psi.$$

Hence $\theta = (1/n)\tan^{-1} nu - \psi$, or

$$\tan n\phi = \tan n(\theta+\psi) = nu = n\left(\frac{b^2-r^2}{n^2r^2-b^2}\right)^{\frac{1}{2}}.$$

Then

$$1+\tan^2 n\phi = \sec^2 n\phi = (n^2-1)b^2/(n^2r^2-b^2).$$

or

$$r^2 = \frac{b^2}{n^2}(1+(n^2-1)\cos^2 n\phi)$$

$$= b^2\cos^2 n\phi + \frac{b^2}{n^2}\sin^2 n\phi. \quad (25)$$

From Eq. (24) we have $\omega_P r \sin\psi/(b^2-r^2)^{\frac{1}{2}} = \omega_L$. Substituting this in the second term of the left-hand member of Eq. (23), and using the right-hand members of Eqs. (21) and (22), we obtain

$$\omega_P(b^2-r^2)^{\frac{1}{2}}\frac{d}{dr}(r\sin\psi) - r\omega_L = -2r\omega_L,$$

or

$$\frac{d\phi}{dt} = \frac{ds}{dt}\frac{d\phi}{ds} = -\omega_L,$$

whence

$$\phi = -\omega_L t. \quad (26)$$

Thus the time-rate of change of the direction of the bob in motion along the Foucault pendulum star path is constant. By introducing this value of $\phi$ into Eq. (25), we check Eq. (19), finding

$$r^2 = \frac{b^2}{n^2} + b^2\left(1-\frac{1}{n^2}\right)\cos^2 n\omega_L t. \quad (27)$$

### The $n$-Leaved Rose

If the constant of integration in Eq. (24) is made zero instead of $\omega_L b^2$, we obtain

$$\omega_P(b^2-r^2)^{\frac{1}{2}}\sin\psi = -r\omega_L. \quad (28)$$

Remembering that $\sin\psi = r\,d\theta/(dr^2+r^2d\theta^2)^{\frac{1}{2}}$, we may solve Eq. (28) for $d\theta$ and obtain

$$d\theta = -\omega_L dr/[b^2\omega_P^2 - (\omega_P^2+\omega_L^2)r^2]^{\frac{1}{2}},$$

whence

$$\theta = \frac{\omega_L}{(\omega_P^2+\omega_L^2)^{\frac{1}{2}}}\cos^{-1}\frac{(\omega_P^2+\omega_L^2)^{\frac{1}{2}}}{b\omega_P}r$$

$$= \frac{1}{n}\cos^{-1}\frac{nr}{b(n^2-1)^{\frac{1}{2}}},$$

and

$$r = b\left(1-\frac{1}{n^2}\right)^{\frac{1}{2}}\cos n\theta \quad \text{or} \quad r = b\cos n\theta, \quad (29)$$

approximately, since $n$ is of the order $10^4$.

Equation (29) will be recognized as that of an $n$-leaved rose. With the help of Eqs. (24) and (28) we may compare it with that of the Foucault star path, Eq. (27). Since $\omega_P(b^2-r^2)^{\frac{1}{2}} = v = ds/dt$ and $\sin\psi = r\,d\theta/ds$, we may write Eq. (24) in the form

$$r^2 d\theta/dt = \omega_L(b^2-r^2).$$

This will be recognized as the area integral, which is constant in central force theory, but here is modified by the Coriolis term $\omega_L r^2$. When this term is removed by rotating the axes, using Eq. (9) as before, we get the Chevilliet ellipse having the foregoing $\omega_L b^2$ for its constant of areas. By making this constant of areas zero, as we did to obtain Eq. (28) from Eq. (24), we are treating a modification of the Chevilliet ellipse motion whereby it reduces to rectilinear simple harmonic motion with a zero-valued constant of areas as viewed in fixed coordinates.

Physically, this situation is realized when we start the Foucault pendulum swinging, not from rest on the rotating earth at the circumference of the circle of radius $b$, but from rest in the fixed coordinates, that is, by giving it an initial velocity $-\omega_L r$ to compensate the effect of the earth's rotation. This velocity amounts to only $\pi/21{,}600$ ft/sec, or about 6 in./hr, in middle latitudes where $\omega_L = 2\pi/T_L = \pi/18$ rad/hr.

By Eq. (21) and $\sin\psi = r\,d\theta/ds$, Eq. (28) can be written, $d\theta/dt = -\omega_L$, whence

$$\theta = -\omega_L t. \quad (30)$$

This indicates that the time-rate of change of direction of the radius vector in the $n$-leaved rose path is constant and equal to the magnitude of the earth's rotation component at the latitude

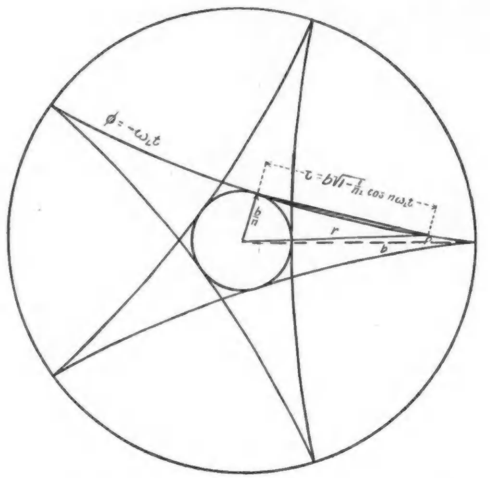


FIG. 2. The Foucault pendulum star path for $n=5$ instead of actual $n=10{,}000$.

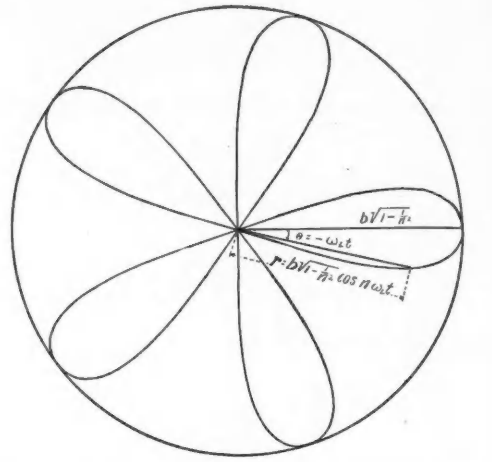


FIG. 3. The Foucault pendulum rose path for $n=5$, showing its radius vector equal to the tangent to the small circle from $P$ on the star path.

in question. This is to be expected for rectilinear swings in fixed space, when viewed on the rotating earth. Substituting Eq. (30) in Eq. (29), we obtain

$$r=b\left(1-\frac{1}{n^2}\right)^{\frac{1}{2}}\cos n\omega_L t \tag{31}$$

for the equation of the $n$-leaved rose.

## $n$-Leaved Rose Is Path on Rotating Earth of Rectilinear S.H.M. in Fixed Coordinates

We noted how the $n$-leaved rose was obtained from Eq. (28) by putting zero for the constant of areas term of the Chevilliet ellipse path in fixed coordinates, implying rectilinear simple harmonic motion for this special degenerate case. This result may be checked by choosing $A$ and $B$ of Eqs. (5) to match the boundary conditions corresponding to Eq. (29) or (31). These conditions are that for $t=0$ we have a maximum value of $r$ at the point whose Cartesian coordinates are $[b(1-1/n^2)^{\frac{1}{2}}, 0]$ and where the components of the velocity are $v_x=0$ and $v_y=-r\omega_L=-\omega_L b \times(1-1/n^2)^{\frac{1}{2}}$. Thus introducing $t=0$ in Eq. (5), we obtain

$$u(0)=x(0)=b(1-1/n^2)^{\frac{1}{2}}=A+B,$$

$$\frac{du}{dt}(0)=iv_y(0)=-i\omega_L b(1-1/n^2)^{\frac{1}{2}}=iA\alpha-iB\beta,$$

whence

$$A\alpha-B\beta=-\omega_L(A+B),$$

or

$$A(\alpha+\omega_L)=B(\beta-\omega_L).$$

Therefore, by Eq. (4a), $A=B$. Thus

$$A=B=\tfrac{1}{2}b(1-1/n^2)^{\frac{1}{2}},$$

and, from Eq. (5),

$$u=\tfrac{1}{2}b(1-1/n^2)^{\frac{1}{2}}(e^{i\alpha t}+e^{-i\beta t}). \tag{32}$$

On separating into real and imaginary parts, we have

$$\begin{aligned} x&=\tfrac{1}{2}b(1-1/n^2)^{\frac{1}{2}}(\cos\alpha t+\cos\beta t),\\ y&=\tfrac{1}{2}b(1-1/n^2)^{\frac{1}{2}}(\sin\alpha t-\sin\beta t). \end{aligned} \tag{33}$$

Squaring and adding and remembering that $\alpha+\beta=2n\omega_L$, by Eqs. (4a) and (15), we get

$$\begin{aligned} r^2=x^2+y^2&=\tfrac{1}{4}b^2(1-1/n^2)[2+2\cos 2n\omega_L t]\\ &=b^2(1-1/n^2)\cos^2 n\omega_L t, \end{aligned} \tag{34}$$

which is the same as Eqs. (29) and (31). If Eqs. (33) are referred to fixed (primed) coordinates according to Eq. (9), we have, in view of Eqs. (4a),

$$\begin{aligned} x'&=\tfrac{1}{2}b(1-1/n^2)^{\frac{1}{2}}[\cos(\alpha+\omega_L)t+\cos(\beta-\omega_L)t]\\ &=b(1-1/n^2)^{\frac{1}{2}}\cos n\omega_L t, \end{aligned} \tag{35}$$

$$y'=\tfrac{1}{2}b(1-1/n^2)^{\frac{1}{2}}[\sin(\alpha+\omega_L)t-\sin(\beta-\omega_L)t]\equiv 0.$$

Thus Eqs. (33) and (34) or (35) represent motion which in the fixed (primed) coordinates is rectilinear simple harmonic along the line $y'=0$, a path which is the modification of the Chevilliet ellipse that occurs when negative rotation to offset the earth's rotation is imparted at the start of a swing instead of starting the pendulum from rest at one of the cusps or star points.

## Construction of the Star Path

The Foucault pendulum star path is related to the $n$-leaved rose and may be constructed from it as follows: (i) the length of the tangent from any point on the star path to the circle $r=b/n$ is equal to the corresponding radius vector of the $n$-leaved rose, and (ii) the inclination of the star path at this point is equal to that of the radius vector of the rose.

The foregoing relation between the $n$-leaved rose and the Foucault pendulum star path is revealed by two pairs of equations already derived, namely,

$$r^2=b^2/n^2+b^2(1-1/n^2)\cos^2 n\omega_L t, \qquad \text{(Star path)} \quad (27)$$

$$r=b(1-1/n^2)^{\frac{1}{2}}\cos n\omega_L t, \qquad (n\text{-leaved rose}) \quad (31)$$

and

$$\phi=\theta+\psi=-\omega_L t, \qquad \text{(Star path)} \quad (26)$$

$$\theta=-\omega_L t. \qquad (n\text{-leaved rose}) \quad (30)$$

The radius vector of the star path, Eq. (27), is evidently the vector sum of the two perpendicular components shown in Fig. 2, one of which, $b/n$, is the radius of the small circle, and the other, $b(1-1/n^2)^{\frac{1}{2}}\cos n\omega_L t$, is the tangent to this small circle from a point $P$ on the path. We see from Eq. (31) that the length of this tangent is equal to the length of the radius vector of the $n$-leaved rose, for the same $t$.

Comparison of Figs. 2 and 3 also shows that the star path inclination given by Eq. (26) at any such point is equal to the inclination of the rose's radius vector given by Eq. (30). The two paths have the same slope only where the rose crosses the origin and $n\omega_L t$ is an odd multiple of $\pi/2$, and the star path touches the small circle. The first of these places is where $-\phi=\omega_L t=\pi/2n$ $=$(approximately) $\pi T/2T_L$; thus the path inclination, or slope, at this first point where the star path touches the small circle is $\pi/2$ times the ratio of the diameters of the two circles, this ratio being of the order of $10^{-4}$.

Although Figs. 2 and 3 look different, it is to be noted that with $n=10^4$ the inner circle's radius is only 0.01 percent of $b$, the amplitude of the equivalent simple pendulum's swing, so that the paths of Figs. 2 and 3 are indistinguishable in an ordinary case. Recall also that it takes an initial velocity of only about 6 in./hr around the larger circle to compensate for the earth's initial motion, and then Eq. (31) will be the equation of the actual path. But this velocity is so minute that it might be imparted by accident, and constitutes a small effect that is not unlikely to be imparted to the Foucault pendulum bob when its swing is started without accurate methods, such as burning a string used to displace it from its central position. Thus the $n$-leaved rose is indistinguishable from the Foucault star path without precision instruments, and without such methods of starting the swing, it is not unlikely to be the actual path.

The opinions or assertions contained in this article are the private ones of the writer and are not to be construed as official or reflecting the views of the Navy Department, or the Naval Service at large.

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*A GREAT deal of the joy of life consists in doing perfectly, or at least to the best of one's ability, everything which he attempts to do. There is a sense of satisfaction, a pride in surveying such a work—a work which is rounded, full, exact, complete in all its parts—which the superficial man, who leaves his work in a slovenly, slipshod, half-finished condition, can never know. It is this conscientious completeness which turns work into art. The smallest thing, well done, becomes artistic.*—William Mathews.

# Multiple Reflections from Plane Mirrors

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THE treatment of multiple reflections by plane mirrors, if mentioned in textbooks on physics, is nearly always confined to one or the other of two special cases. Either the two plane mirrors are parallel and facing each other, in which case an infinite number of images of diminishing brightness can be seen; or the two facing mirrors form a dihedral angle $\theta$ of either 90° or 60°, which divides into 360° an even integral number of times. No mention is made of the case where $n$ $[=360°/\theta]$ may be an odd integer, let alone the general case where $n$ is a mixed number such as 3.24. And the further question of how many of these $n$ images an observer in the object space can actually see was not mentioned in a single book studied. Probably these cases are too advanced for elementary textbooks and do not have sufficient practical application to warrant space in more advanced books.

A cursory inspection of the books in a typical physics library indicated that only two authors treated the general case—Southall's *Mirrors, prisms and lenses* (1918, 1923, 1933) and Watson's *Textbook of physics* (1920). The treatment in Watson's second edition (1900) is not complete and in fact is partly in error. Houstoun, in his *Treatise on light* (1928), states that he gives Watson's discussion, but he quotes the incomplete one. The discussions in both editions of Duncan and Starling's *Textbook of Physics* (1920 and 1922) are the same, and both are partly wrong. Edser's *Light for students* (1903) discusses only the special case of $n$ an even integer.

Multiple reflection is mentioned in six of 24 other standard college text books, and these six discuss only one or the other special case, or both, but never the general one. The statement that when $n$ is an integer there are $n-1$ images is the most common one. As we shall see, this generalization is incorrect when $n$ is an odd integer. Most advanced books omit the topic entirely.

The subject can be discussed more easily and yet covered completely by dividing the possible cases into two main groups, each with two cases. The reader is advised that the subject will be more fascinating as well as easier to follow if, before reading further, he will provide himself with a pair of mirrors and some small objects, of different colors or otherwise easily distinguishable, which he can place at various points in the object space.

## GROUP I: *n* AN INTEGER

When $n$ is an integer the space about the vertex of the dihedral angle can be divided into exactly $n$ equal spaces. One of these is the object space between the two mirrors. Each of the other spaces will contain one image of the object space, except that the space behind both mirrors extended will contain two complete sets of images, making the *total* number of images $n$.

The observer, who is also located somewhere in the object space, can, by looking in the proper direction, *always* see *all* of the images except those behind both mirrors extended, but whether he can see none, one or both of the latter images depends on his location as well as on the location of the images themselves.

### Case A: *n* an Odd Integer

When $n$ is an odd integer there will be $n$ separate images because the two sets located behind both mirrors extended do not coincide. To determine how many of these images can be seen, consider Fig. 1, where $\theta=72°$ and $n=5$. In this and subsequent figures the image due to a reflection in mirror **A** alone is called $A$, that due to a reflection in **B** alone is called $B$. An image produced by reflection first in mirror **A**, then in mirror **B** is called $AB$, and one involving a third reflection in mirror **A** would be called $ABA$ or, more likely, $ABA'$, since in dropping the perpendicular from image $AB$ to mirror **A**, to locate image $ABA'$, the perpendicular would be more apt to intersect the extension of mirror **A**, which is labeled **A′** in the figure.

Now from the place $E_1$, look toward the two images $BA'$ and $ABA'$ of the *small* circle (between **A** and **B**, near **A**). Both lines of sight, shown in Fig. 1, intersect mirror **A**; consequently from $E_1$ both $BA'$ and $ABA'$ can be seen. But, if the point of observation is changed to $E_2$, the line of sight to $ABA'$ would intersect mirror **B**, and so this image cannot be seen from here. If the observer moves to $E_3$, neither of the images of the small circle can be seen, but now both images $AB'$ and

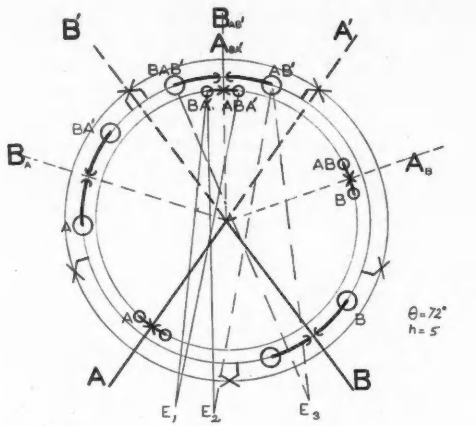


FIG. 1. Two plane mirrors **A** and **B** forming a dihedral angle of 72°, their extensions labeled **A′** and **B′**. The reflections of the mirrors in one another are shown, as well as the five separate images of each object. Of these five images, two are behind both mirrors extended, and the observer can see one or both or neither of them depending on his viewing point and the region under inspection; the other three images of any object can always be seen, from any viewing point, by looking in the proper direction.

$BAB'$ of the *large* circle can be seen. Consequently, as an observer moves from position $E_1$, near mirror **A**, through position $E_2$, on to position $E_3$, he will be able to see a *total* of first $n$, then $n-1$, then $n-2$ images of a certain region of the object space, while for another region of the object space he will see $n-2$ images from position $E_1$; then $n-1$ and finally $n$ images as he gets farther over. In any event, he can see $n$, $n-1$ or $n-2$ *of the total of n images formed*, depending upon his point of observation and the region viewed.

## Case B: $n$ an Even Integer

When $n$ is an even integer such as 4 or 6, the two sets of images in the space behind both mirrors extended coincide in every detail, so that there appear only $n-1$ separate images. Most books stop with this case, making the statement that when $n$ is an *integer* there are $n-1$ images.

As one looks at Fig. 2, where $\theta=90°$, the coincident images $BA'$ and $AB'$ of the circle and of the flag are easily traced. From the position $E_1$, the lines of sight to both intersect mirror **A**; consequently it is the image $BA'$ that is seen in each case. From $E_2$, it is the $BA'$-image of the circle but the $AB'$-image of the flag; and from $E_3$, it is the $AB'$-image of each. The dashed and

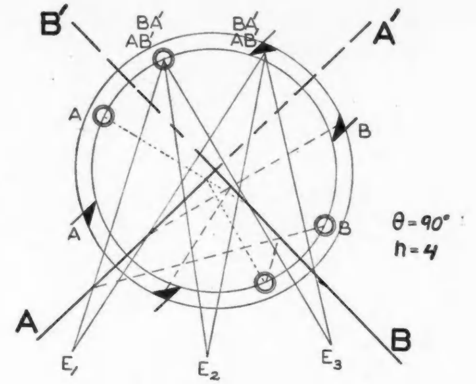


FIG. 2. Two plane mirrors forming an angle of 90°. There are four images, but the two behind both mirrors extended coincide.

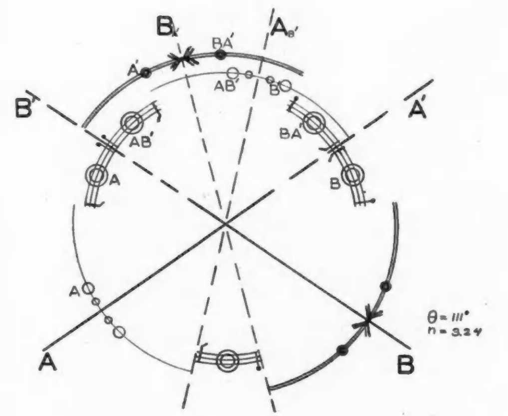


FIG. 3. Two plane mirrors forming an angle of 111° so that $n=3.24$. There are three images of the object space near the mirrors, and four images of the central portion.

dotted lines will help one to trace the actual light paths in some cases. Since, however, the images coincide, the shift from one to the other cannot be noticed as the viewing point moves from $E_1$ to $E_3$.

Also, it is evidently not possible to see both coincident images of a given object point from any one place; nor is there any place between the mirrors from which both are invisible; consequently, the pattern remains fixed and unchanging no matter how the observer moves, and this leads to the statement that there are $n-1$ images of each object. Actually there are a total of $n$ images, but only one of the two coincident ones can be seen at any one time.

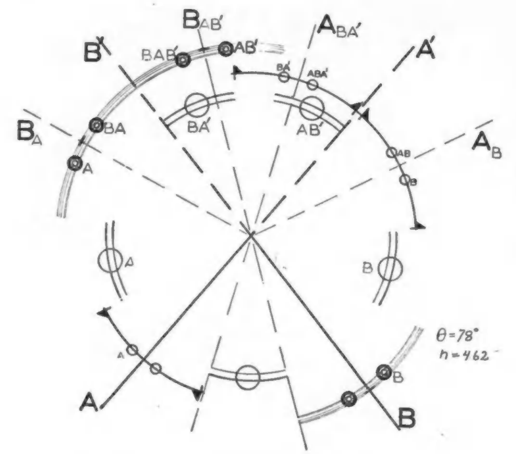


FIG. 4. Two plane mirrors forming an angle of 78° so that $n=4.62$. There are four images of the central portion of the object space, and five images of the portions adjacent to the mirrors.

### GROUP II: $n$ A MIXED NUMBER

When $n$ is a mixed number such as 3.24 or 8.56, it is found that there are not the same number of images of all points in the object space. Call $m$ the integral part of $n$. There will be $m$ images, three or eight, of all the space, but there will be an additional image of that fraction of the object space indicated by the decimal fraction $n-m$. For $n=3.24$ there would be *three* images of all the space and *four* images of 0.24 of it; for $n=8.56$, *eight* images of all the space and *nine* images of 0.56 of it.

#### Case C: $m$ is Odd

When $m$, the integral part of $n$, is odd, the extra image is that of the central part of the object space (see Fig. 3). The image space behind both mirrors still has two and only two complete sets of noncoincident images, of which both, or one, or neither can be seen, depending on the viewing point and the region under inspection. The additional image is not in this space but in the regions between **A** and **B′** or **B** and **A′**. In Fig. 3, the images formed by mirror **A** of the small open circles near **A** are in the region between **A** and **B′**; but when formed by **B**, they are in the space behind both mirrors. Likewise, the small heavy circle and arrow near mirror **B** have images in the region between **B** and **A′** but none in the region between **A** and **B′**. However, the compound circle and flags in the central 0.24 of the object space are mirrored once in mirror **A** between **A** and **B′**, once in mirror **B** between **B** and **A′** and twice behind both mirrors, giving a total of four images for this central region. When $m$ is odd, one can see $m$, $m-1$ or $m-2$ images of the total of $m$ or $m+1$ images produced, but one can never see all $m+1$ images of the central portion.

#### Case D: $m$ is Even

When $m$ is even, as in the case where $\theta=78°$ and $n=4.62$, there will be *four* images of all the object space and *five* images of 0.62 of it; only now the 0.62 consists of two regions, one of 0.31 of the object space adjacent to each mirror. In this case, the *central* portion has the *fewer* images, and the portions next the mirrors give the larger number of images (see Fig. 4). Of these $m$ or $m+1$ images, the observer is able to see $m+1$, $m$, or $m-1$, depending of course on his position and the region under inspection.

The whole problem is fascinating, for, of course, two parallel mirrors, for which $\theta=0$ and therefore $n=\infty$, is a special case at one end of the sequence, and the single plane mirror can be considered a special case at the other end of the sequence, with $\theta=180°$ and $n=2$. To study the single mirror, let the dihedral angle $\theta$ gradually increase from 170° to 180°. The two images in the region behind both mirrors will gradually approach each other and become coincident when $\theta$ is exactly 180°. The *two* mirrors then can be considered parts of one large plane mirror.

Doubtless there is nothing new in this discussion, but it is hoped that it will prove of value because the subject is omitted entirely or treated less completely in both elementary and advanced books.

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*Accuracy is the twin brother of honesty.*—TRYON EDWARDS.

# The Discovery of X-Rays

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Wilhelm Conrad Röntgen was born on March 27, 1845. He discovered x-rays on November 8, 1895. During the year 1945 there occurs therefore both the centennial of the birth of the discoverer and the semicentennial of the discovery of x-rays. It is fitting to celebrate this interesting double anniversary by publishing a facsimile reproduction of Röntgen's "preliminary communication." This short paper is one of the classics of experimental physics. Röntgen's "second communication" is also reproduced because of the importance for the future of physics of the discovery there recorded, even though Röntgen himself failed to follow up this important lead.

The discovery of x-rays may be said to mark the beginning of "the new era in physics," a period in which startling discoveries and advances have taken place with such bewildering rapidity that they cannot yet be seen in their true historical perspective. That the discovery was, however, the fruit of many earlier efforts is clearly recognized[1] and detracts in no way from Röntgen's claim to fame.

Working in his laboratory at the University of Würzburg, Röntgen made his memorable discovery on November 8, 1895. He handed the manuscript of his "preliminary communication" to the president of the Physikalisch-medicinischen Gesellschaft of Würzburg on December 28, 1895, less than eight weeks later. It was immediately printed in the *Sitzungsberichte* of the society for that year (pages 132–141). The publishers of the *Sitzungsberichte* had a number of reprints struck off and issued in yellow wrappers without title-page, but with the date "Ende 1895." This first separate edition of Röntgen's paper apparently was exhausted immediately for, during 1896, second, third, fourth and fifth editions of it were issued. It was soon translated into many languages and became known all over the world. On March 9, 1896, Röntgen submitted his second communication to the Physikalisch-medicinischen Gesellschaft, and it was published in the *Sitzungsberichte* for 1896 (pages 11–19) with a plate of an x-ray photograph of Professor Kölliker's hand. The publisher again issued a reprint, but without the plate. The photostats here reproduced are from the original printings in the *Sitzungsberichte*. The translations are those made by Professor George F. Barker of the University of Pennsylvania.[2]

There is an interesting story associated with the photograph of Professor Kölliker's hand. Röntgen spoke publicly on his discovery for the first time on January 23, 1896, before the Würzburger Physikalisch-medicinischen Gesellschaft. Glasser[3] describes the incident as follows:

> After the brilliant demonstration and amid the great enthusiasm of the audience, Röntgen asked the famous anatomist of the university, His Excellency A. von Kölliker, for permission to photograph his hand. Von Kölliker eagerly complied with this request and when the excellent picture was shown a little later, there was tremendous applause, and all present felt that this was a moment of real historical significance.

It is unnecessary here to review Röntgen's

Wilhelm Conrad Röntgen (1845–1923).

[1] See especially G. Sarton, "The discovery of x-rays," *Isis* 26, 349 (1937).

[2] Harper's Scientific Memoir, *Röntgen rays* (New York, 1898).

[3] O. Glasser, *Wilhelm Conrad Röntgen* (Springfield, Illinois, 1934), p. 50.

life and work, as this has been done most ably by GLASSER in the biography from which the foregoing quotation was taken. This excellent work contains also a useful bibliography.

Am 28. Dezember wurde als Beitrag eingereicht.

**W. C. Röntgen: Ueber eine neue Art von Strahlen.**

(Vorläufige Mittheilung.)

1. Lässt man durch eine *Hittorf*'sche Vacuumröhre, oder einen genügend evacuirten *Lenard*'schen, *Crookes*'schen oder ähnlichen Apparat die Entladungen eines grösseren *Ruhmkorff*'s gehen und bedeckt die Röhre mit einem ziemlich eng anliegenden Mantel aus dünnem schwarzem Carton, so sieht man in dem vollständig verdunkelten Zimmer einen in die Nähe des Apparates gebrachten, mit Bariumplatincyanür angestrichenen Papierschirm bei jeder Entladung hell aufleuchten, fluoresciren, gleichgültig ob die angestrichene oder die andere Seite des Schirmes dem Entladungsapparat zugewendet ist. Die Fluorescenz ist noch in 2 m Entfernung vom Apparat bemerkbar.

Man überzeugt sich leicht, dass die Ursache der Fluorescenz vom Entladungsapparat und von keiner anderen Stelle der Leitung ausgeht.

2. Das an dieser Erscheinung zunächst Auffallende ist, dass durch die schwarze Cartonhülse, welche keine sichtbaren oder ultravioletten Strahlen des Sonnen- oder des elektrischen Bogenlichtes durchlässt, ein Agens hindurchgeht, das im Stande ist, lebhafte Fluorescenz zu erzeugen, und man wird deshalb wohl zuerst untersuchen, ob auch andere Körper diese Eigenschaft besitzen.

Man findet bald, dass alle Körper für dasselbe durchlässig sind, aber in sehr verschiedenem Grade. Einige Beispiele führe ich an. Papier ist sehr durchlässig:[1]) hinter einem eingebun-

[1]) Mit „Durchlässigkeit" eines Körpers bezeichne ich das Verhältniss der Helligkeit eines dicht hinter dem Körper gehaltenen Fluorescenzschirmes zu derjenigen Helligkeit des Schirmes, welcher dieser unter denselben Verhältnissen aber ohne Zwischenschaltung des Körpers zeigt.

Röntgen: Ueber eine neue Art von Strahlen. 133

denen Buch von ca. 1000 Seiten sah ich den Fluorescenzschirm noch deutlich leuchten; die Druckerschwärze bietet kein merkliches Hinderniss. Ebenso zeigte sich Fluorescenz hinter einem doppelten Whistspiel; eine einzelne Karte zwischen Apparat und Schirm gehalten macht sich dem Auge fast gar nicht bemerkbar. — Auch ein einfaches Blatt Stanniol ist kaum wahrzunehmen; erst nachdem mehrere Lagen über einander gelegt sind, sieht man ihren Schatten deutlich auf dem Schirm. — Dicke Holzblöcke sind noch durchlässig; zwei bis drei cm dicke Bretter aus Tannenholz absorbiren nur sehr wenig. — Eine ca. 15 mm dicke Aluminiumschicht schwächte die Wirkung recht beträchtlich, war aber nicht im Stande, die Fluorescenz ganz zum Verschwinden zu bringen. — Mehrere cm dicke Hartgummischeiben lassen noch Strahlen[1]) hindurch. — Glasplatten gleicher Dicke verhalten sich verschieden, je nachdem sie bleihaltig sind (Flintglas) oder nicht; erstere sind viel weniger durchlässig als letztere. — Hält man die Hand zwischen den Entladungsapparat und den Schirm, so sieht man die dunkleren Schatten der Handknochen in dem nur wenig dunklen Schattenbild der Hand. — Wasser, Schwefelkohlenstoff und verschiedene andere Flüssigkeiten erweisen sich in Glimmergefässen untersucht als sehr durchlässig. — Dass Wasserstoff wesentlich durchlässiger wäre als Luft habe ich nicht finden können. — Hinter Platten aus Kupfer, resp. Silber, Blei, Gold, Platin ist die Fluorescenz noch deutlich zu erkennen, doch nur dann, wenn die Plattendicke nicht zu bedeutend ist. Platin von 0,2 mm Dicke ist noch durchlässig; die Silber- und Kupferplatten können schon stärker sein. Blei in 1,5 mm Dicke ist so gut wie undurchlässig und wurde deshalb häufig wegen dieser Eigenschaft verwendet. — Ein Holzstab mit quadratischem Querschnitt (20 × 20 mm), dessen eine Seite mit Bleifarbe weiss angestrichen ist, verhält sich verschieden, je nachdem er zwischen Apparat und Schirm gehalten wird; fast vollständig wirkungslos, wenn die X-Strahlen parallel der angestrichenen Seite durchgehen, entwirft der Stab einen dunklen Schatten, wenn die Strahlen die Anstrichfarbe durchsetzen müssen. — In eine ähnliche Reihe, wie die Metalle, lassen sich ihre Salze, fest oder in Lösung, in Bezug auf ihre Durchlässigkeit ordnen.

[1]) Der Kürze halber möchte ich den Ausdruck „Strahlen" und zwar zur Unterscheidung von anderen den Namen „X-Strahlen" gebrauchen. Vergl. u. p. 140.

134 Sitzungsberichte der physikal.-medicin. Gesellschaft. Jahrg. 1895.

3. Die angeführten Versuchsergebnisse und andere führen zu der Folgerung, dass die Durchlässigkeit der verschiedenen Substanzen, gleiche Schichtendicke vorausgesetzt, wesentlich bedingt ist durch ihre Dichte: keine andere Eigenschaft macht sich wenigstens in so hohem Grade bemerkbar als diese.

Dass aber die Dichte doch nicht ganz allein massgebend ist, das beweisen folgende Versuche. Ich untersuchte auf ihre Durchlässigkeit nahezu gleichdicke Platten aus Glas, Aluminium, Kalkspath und Quarz; die Dichte dieser Substanzen stellte sich als ungefähr gleich heraus, und doch zeigte sich ganz evident, dass der Kalkspath beträchtlich weniger durchlässig ist als die übrigen Körper, die sich untereinander ziemlich gleich verhielten. Eine besonders starke Fluorescenz des Kalkspathes (vergl. u. pag. 135) namentlich im Vergleich zum Glas habe ich nicht bemerkt.

4. Mit zunehmender Dicke werden alle Körper weniger durchlässig. Um vielleicht eine Beziehung zwischen Durchlässigkeit und Schichtendicke finden zu können, habe ich photographische Aufnahmen (vergl. u. pag. 135) gemacht, bei denen die photographische Platte zum Theil bedeckt war mit Stanniolschichten von stufenweise zunehmender Blätterzahl; eine photometrische Messung soll vorgenommen werden, wenn ich im Besitz eines geeigneten Photometers bin.

5. Aus Platin, Blei, Zink und Aluminium wurden durch Auswalzen Bleche von einer solchen Dicke hergestellt, dass alle nahezu gleich durchlässig erschienen. Die folgende Tabelle enthält die gemessene Dicke in mm, die relative Dicke bezogen auf die des Platinbleches und die Dichte.

| | Dicke | relat. Dicke | Dichte |
|---|---|---|---|
| Pt. | 0,018 mm | 1 | 21,5 |
| Pb. | 0,05 „ | 3 | 11,3 |
| Zn. | 0,10 „ | 6 | 7,1 |
| Al. | 3,5 „ | 200 | 2,6 |

Aus diesen Werthen ist zu entnehmen, dass keineswegs gleiche Durchlässigkeit verschiedener Metalle vorhanden ist, wenn das Produkt aus Dicke und Dichte gleich ist. Die Durchlässigkeit nimmt in viel stärkerem Masse zu, als jenes Product abnimmt.

Röntgen. Ueber eine neue Art von Strahlen. 135

6. Die Fluorescenz des Bariumplatincyanürs ist nicht die einzige erkennbare Wirkung der X-Strahlen. Zunächst ist zu erwähnen, dass auch andere Körper fluoresciren; so z. B. die als Phosphore bekannten Calciumverbindungen, dann Uranglas, gewöhnliches Glas, Kalkspath, Steinsalz etc.

Von besondererer Bedeutung in mancher Hinsicht ist die Thatsache, dass photographische Trockenplatten sich als empfindlich für die X-Strahlen erwiesen haben. Man ist im Stande manche Erscheinung zu fixiren, wodurch Täuschungen leichter ausgeschlossen werden; und ich habe, wo es irgend anging, jede wichtigere Beobachtung, die ich mit dem Auge am Fluorescenzschirm machte, durch eine photographische Aufnahme controllirt.

Dabei kommt die Eigenschaft der Strahlen, fast ungehindert durch dünnere Holz-, Papier- und Stanniolschichten hindurchgehen zu können, sehr zu Statten; man kann die Aufnahmen mit der in der Cassette, oder in einer Papierumhüllung eingeschlossenen photographischen Platte im beleuchteten Zimmer machen. Andererseits hat diese Eigenschaft auch zur Folge, dass man unentwickelte Platten nicht bloss durch die gebräuchliche Hülle aus Pappendeckel und Papier geschützt längere Zeit in der Nähe des Entladungsapparates liegen lassen darf.

Fraglich erscheint es noch, ob die chemische Wirkung auf die Silbersalze der photographischen Platte direct von den X-Strahlen ausgeübt wird. Möglich ist es, dass diese Wirkung herrührt von dem Fluorescenzlicht, das, wie oben angegeben, in der Glasplatte, oder vielleicht in der Gelatineschicht erzeugt wird. „Films" können übrigens ebenso gut wie Glasplatten verwendet werden.

Dass die X-Strahlen auch eine Wärmewirkung auszuüben im Stande sind, habe ich noch nicht experimentell nachgewiesen; doch darf man wohl diese Eigenschaft als vorhanden annehmen, nachdem durch die Fluorescenzerscheinungen die Fähigkeit der X-Strahlen, verwandelt zu werden, nachgewiesen ist, und es sicher ist, dass nicht alle auffallenden X-Strahlen den Körper als solche wieder verlassen.

Die Retina des Auges ist für unsere Strahlen unempfindlich; das dicht an den Entladungsapparat herangebrachte Auge bemerkt nichts, wiewohl nach den gemachten Erfahrungen die im Auge enthaltenen Medien für die Strahlen durchlässig genug sein müssen.

7. Nachdem ich die Durchlässigkeit verschiedener Körper von relativ grosser Dicke erkannt hatte, beeilte ich mich, zu erfahren, wie sich die X-Strahlen beim Durchgang durch ein Prisma verhalten, ob sie darin abgelenkt werden oder nicht. Versuche mit Wasser und Schwefelkohlenstoff in Glimmerprismen von ca. 30° brechendem Winkel haben gar keine Ablenkung erkennen lassen weder am Fluorescenzschirm, noch an der photographischen Platte. Zum Vergleich wurde unter denselben Verhältnissen die Ablenkung von Lichtstrahlen beobachtet; die abgelenkten Bilder lagen auf der Platte um ca. 10 mm resp. ca. 20 mm von dem nicht abgelenkten entfernt. — Mit einem Hartgummi- und einem Aluminiumprisma von ebenfalls ca. 30° brechendem Winkel habe ich auf der photographischen Platte Bilder bekommen, an denen man vielleicht eine Ablenkung erkennen kann. Doch ist die Sache sehr unsicher, und die Ablenkung ist, wenn überhaupt vorhanden, jedenfalls so klein, dass der Brechungsexponent der X-Strahlen in den genannten Substanzen höchstens 1.05 sein könnte. Mit dem Fluorescenzschirm habe ich auch in diesem Fall keine Ablenkung beobachten können.

Versuche mit Prismen aus dichteren Metallen lieferten bis jetzt wegen der geringen Durchlässigkeit und der in Folge dessen geringen Intensität der durchgelassenen Strahlen kein sicheres Resultat.

In Anbetracht dieser Sachlage einerseits und andererseits der Wichtigkeit der Frage, ob die X-Strahlen beim Uebergang von einem Medium zum anderen gebrochen werden können oder nicht, ist es sehr erfreulich, dass diese Frage noch in anderer Weise untersucht werden kann, als mit Hülfe von Prismen. Fein pulverisirte Körper lassen in genügender Schichtendicke das auffallende Licht nur wenig und zerstreut hindurch in Folge von Brechung und Reflexion; erweisen sich nun die Pulver für die X-Strahlen gleich durchlässig, wie die cohärente Substanz — gleiche Massen vorausgesetzt — so ist damit nachgewiesen, dass sowohl eine Brechung als auch eine regelmässige Reflexion nicht in merklichem Betrage vorhanden ist. Die Versuche wurden mit fein pulverisirtem Steinsalz, mit feinem, auf electrolytischem Wege gewonnenem Silberpulver und dem zu chemischen Untersuchungen vielfach verwandten Zinkstaub angestellt, es ergab sich in allen Fällen kein Unterschied in der Durchlässigkeit der



Pulver und der cohärenten Substanz, sowohl bei der Beobachtung am Fluorescenzschirm, als auch auf der photographischen Platte.

Dass man mit Linsen die X-Strahlen nicht concentriren kann, ist nach dem Mitgetheilten selbstverständlich; eine grosse Hartgummilinse und eine Glaslinse erwiesen sich in der That als wirkungslos. Das Schattenbild eines runden Stabes ist in der Mitte dunkler als am Rande; dasjenige einer Röhre, die mit einer Substanz gefüllt ist, die durchlässiger ist als das Material der Röhre, ist in der Mitte heller als am Rande.

8. Die Frage nach der Reflexion der X-Strahlen ist durch die Versuche des vorigen Paragraphen als in dem Sinne erledigt zu betrachten, dass eine merkliche regelmässige Zurückwerfung der Strahlen an keiner der untersuchten Substanzen stattfindet. Andere Versuche, die ich hier übergehen will, führen zu demselben Resultat.

Indessen ist eine Beobachtung zu erwähnen, die auf den ersten Blick das Gegentheil zu ergeben scheint. Ich exponirte eine durch schwarzes Papier gegen Lichtstrahlen geschützte photographische Platte, mit der Glasseite dem Entladungsapparat zugewendet, den X-Strahlen; die empfindliche Schicht war bis auf einen frei bleibenden Theil mit blanken Platten aus Platin, Blei, Zink und Aluminium in sternförmiger Anordnung bedeckt. Auf dem entwickelten Negativ ist deutlich zu erkennen, dass die Schwärzung unter dem Platin, dem Blei und besonders unter dem Zink stärker ist als an den anderen Stellen; das Aluminium hatte gar keine Wirkung ausgeübt. Es scheint somit, dass die drei genannten Metalle die Strahlen reflectiren, indessen wären noch andere Ursachen für die stärkere Schwärzung denkbar, und um sicher zu gehen, legte ich bei einem zweiten Versuch zwischen die empfindliche Schicht und die Metallplatten ein Stück dünnes Blattaluminium, welches für ultraviolette Strahlen undurchlässig, dagegen für die X-Strahlen sehr durchlässig ist. Da auch jetzt wieder im Wesentlichen dasselbe Resultat erhalten wurde, so ist eine Reflexion von X-Strahlen an den genannten Metallen nachgewiesen.

Hält man diese Thatsache zusammen mit der Beobachtung, dass Pulver ebenso durchlässig sind, wie cohärente Körper, dass weiter Körper mit rauher Oberfläche sich beim Durchgang der X-Strahlen, wie auch bei dem zuletzt beschriebenen Versuch ganz gleich wie polirte Körper verhalten, so kommt man zu der Anschauung, dass zwar eine regelmässige Reflexion, wie gesagt, nicht statt-



findet, dass aber die Körper sich den X-Strahlen gegenüber ähnlich verhalten, wie die trüben Medien dem Licht gegenüber.

Da ich auch keine Brechung beim Uebergang von einem Medium zum anderen nachweisen konnte, so hat es den Anschein, als ob die X-Strahlen sich mit gleicher Geschwindigkeit in allen Körpern bewegen, und zwar in einem Medium, das überall vorhanden ist, und in welchem die Körpertheilchen eingebettet sind. Die letzteren bilden für die Ausbreitung der X-Strahlen ein Hinderniss und zwar im Allgemeinen ein desto grösseres, je dichter der betreffende Körper ist.

9. Demnach wäre es möglich, dass auch die Anordnung der Theilchen im Körper auf die Durchlässigkeit desselben einen Einfluss ausübte, dass z. B. ein Stück Kalkspath bei gleicher Dicke verschieden durchlässig wäre, wenn dasselbe in der Richtung der Axe oder senkrecht dazu durchstrahlt wird. Versuche mit Kalkspath und Quarz haben aber ein negatives Resultat ergeben.

10. Bekanntlich ist *Lenard* bei seinen schönen Versuchen über die von einem dünnen Aluminiumblättchen hindurchgelassenen *Hittorf*'schen Kathodenstrahlen zu dem Resultat gekommen, dass diese Strahlen Vorgänge im Aether sind, und dass sie in allen Körpern diffus verlaufen. Von unseren Strahlen haben wir Aehnliches aussagen können.

In seiner letzten Arbeit hat *Lenard* das Absorptionsvermögen verschiedener Körper für die Kathodenstrahlen bestimmt und dasselbe u. a. für Luft von Atmosphärendruck zu 4,10, 3,40, 3,10 auf 1 cm bezogen gefunden, je nach der Verdünnung des im Entladungsapparat enthaltenen Gases. Nach der aus der Funkenstrecke geschätzten Entladungsspannung zu urtheilen, habe ich es bei meinen Versuchen meistens mit ungefähr gleichgrossen und nur selten mit geringeren und grösseren Verdünnungen zu thun gehabt. Es gelang mir mit dem *L. Weber*'schen Photometer — ein besseres besitze ich nicht — in atmosphärischer Luft die Intensitäten des Fluorescenzlichtes meines Schirmes in zwei Abständen — ca. 100 resp. 200 mm — vom Entladungsapparat mit einander zu vergleichen, und ich fand aus drei recht gut mit einander übereinstimmenden Versuchen, dass dieselben sich umgekehrt wie die Quadrate der resp. Entfernungen des Schirmes vom Entladungsapparat verhalten. Demnach hält die Luft von den hindurchgehenden X-Strahlen einen viel kleineren Bruchtheil zurück als von den Kathodenstrahlen. Dieses Resultat



ist auch ganz in Uebereinstimmung mit der oben erwähnten Beobachtung, dass das Fluorescenzlicht noch in 2 m Distanz vom Entladungsapparat wahrzunehmen ist.

Aehnlich wie Luft verhalten sich im Allgemeinen die anderen Körper: sie sind für die X-Strahlen durchlässiger als für die Kathodenstrahlen.

11. Eine weitere sehr bemerkenswerthe Verschiedenheit in dem Verhalten der Kathodenstrahlen und der X-Strahlen liegt in der Thatsache, dass es mir trotz vieler Bemühungen nicht gelungen ist, auch in sehr kräftigen magnetischen Feldern eine Ablenkung der X-Strahlen durch den Magnet zu erhalten.

Die Ablenkbarkeit durch den Magnet gilt aber bis jetzt als ein characteristisches Merkmal der Kathodenstrahlen; wohl ward von *Hertz* und *Lenard* beobachtet, dass es verschiedene Arten von Kathodenstrahlen gibt, die sich durch „ihre Phosphorescenzerzeugung, Absorbirbarkeit und Ablenkbarkeit durch den Magnet von einander unterscheiden", aber eine beträchtliche Ablenkung wurde doch in allen von ihnen untersuchten Fällen wahrgenommen, und ich glaube nicht, dass man dieses Characteristicum ohne zwingenden Grund aufgeben wird.

12. Nach besonders zu diesem Zweck angestellten Versuchen ist es sicher, dass die Stelle der Wand des Entladungsapparates, die am stärksten fluorescirt, als Hauptausgangspunkt der nach allen Richtungen sich ausbreitenden X-Strahlen zu betrachten ist. Die X-Strahlen gehen somit von der Stelle aus, wo nach den Angaben verschiedener Forscher die Kathodenstrahlen die Glaswand treffen. Lenkt man die Kathodenstrahlen innerhalb des Entladungsapparates durch einen Magnet ab, so sieht man, dass auch die X-Strahlen von einer anderen Stelle, d. h. wieder von dem Endpunkte der Kathodenstrahlen ausgehen.

Auch aus diesem Grund können die X-Strahlen, die nicht ablenkbar sind, nicht einfach unverändert von der Glaswand hindurchgelassene resp. reflectirte Kathodenstrahlen sein. Die grössere Dichte des Glases ausserhalb des Entladungsgefässes kann ja nach *Lenard* für die grosse Verschiedenheit der Ablenkbarkeit nicht verantwortlich gemacht werden.

Ich komme deshalb zu dem Resultat, dass die X-Strahlen nicht identisch sind mit den Kathodenstrahlen, dass sie aber von den Kathodenstrahlen in der Glaswand des Entladungsapparates erzeugt werden.

13. Diese Erzeugung findet nicht nur in Glas statt, sondern, wie ich an einem mit 2 mm starkem Aluminiumblech abgeschlossenen Apparat beobachten konnte, auch in diesem Metall. Andere Substanzen sollen später untersucht werden.

14. Die Berechtigung, für das von der Wand des Entladungsapparates ausgehende Agens den Namen „Strahlen" zu verwenden, leite ich zum Theil von der ganz regelmässigen Schattenbildung her, die sich zeigt, wenn man zwischen den Apparat und den fluorescirenden Schirm (oder die photographische Platte) mehr oder weniger durchlässige Körper bringt.

Viele derartige Schattenbilder, deren Erzeugung mitunter einen ganz besonderen Reiz bietet, habe ich beobachtet und theilweise auch photographisch aufgenommen; so besitze ich z. B. Photographien von den Schatten der Profile einer Thüre, welche die Zimmer trennt, in welchen einerseits der Entladungsapparat, andererseits die photographische Platte aufgestellt waren; von den Schatten der Handknochen, von dem Schatten eines auf einer Holzspule versteckt aufgewickelten Drahtes; eines in einem Kästchen eingeschlossenen Gewichtssatzes; einer Bussole, bei welcher die Magnetnadel ganz von Metall eingeschlossen ist; eines Metallstückes, dessen Inhomogenität durch die X-Strahlen bemerkbar wird; etc.

Für die geradlinige Ausbreitung der X-Strahlen beweisend ist weiter eine Lochphotographie, die ich von dem mit schwarzem Papier eingehüllten Entladungsapparat habe machen können, das Bild ist schwach aber unverkennbar richtig.

15. Nach Interferenzerscheinungen der X-Strahlen habe ich viel gesucht, aber leider, vielleicht nur in Folge der geringen Intensität derselben, ohne Erfolg.

16. Versuche, um zu constatiren, ob elektrostatische Kräfte in irgend einer Weise die X-Strahlen beeinflussen können, sind zwar angefangen aber noch nicht abgeschlossen.

17. Legt man sich die Frage vor, was denn die X-Strahlen, — die keine Kathodenstrahlen sein können — eigentlich sind, so wird man vielleicht im ersten Augenblick, verleitet durch ihre lebhaften Fluorescenz- und chemischen Wirkungen, an ultraviolettes Licht denken. Indessen stösst man doch sofort auf schwerwiegende Bedenken. Wenn nämlich die X-Strahlen ultra-



violettes Licht sein sollten, so müsste dieses Licht die Eigenschaft haben:

a) dass es beim Uebergang aus Luft in Wasser, Schwefelkohlenstoff, Aluminium, Steinsalz, Glas, Zink etc. keine merkliche Brechung erleiden kann;
b) dass es von den genannten Körpern nicht merklich regelmässig reflectirt werden kann;
c) dass es somit durch die sonst gebräuchlichen Mittel nicht polarisirt werden kann;
d) dass die Absorption desselben von keiner anderen Eigenschaft der Körper so beeinflusst wird als von ihrer Dichte.

Das heisst, man müsste annehmen, dass sich diese ultravioletten Strahlen ganz anders verhalten, als die bisher bekannten ultrarothen, sichtbaren und ultravioletten Strahlen.

Dazu habe ich mich nicht entschliessen können und nach einer anderen Erklärung gesucht.

Eine Art von Verwandtschaft zwischen den neuen Strahlen und den Lichtstrahlen scheint zu bestehen, wenigstens deutet die Schattenbildung, die Fluorescenz und die chemische Wirkung, welche bei beiden Strahlenarten vorkommen, darauf hin. Nun weiss man schon seit langer Zeit, dass ausser den transversalen Lichtschwingungen auch longitudinale Schwingungen im Aether vorkommen können und nach Ansicht verschiedener Physiker vorkommen müssen. Freilich ist ihre Existenz bis jetzt noch nicht evident nachgewiesen, und sind deshalb ihre Eigenschaften noch nicht experimentell untersucht.

Sollten nun die neuen Strahlen nicht longitudinalen Schwingungen im Aether zuzuschreiben sein?

Ich muss bekennen, dass ich mich im Laufe der Untersuchung immer mehr mit diesem Gedanken vertraut gemacht habe und gestatte mir dann auch diese Vermuthung hier auszusprechen, wiewohl ich mir sehr wohl bewusst bin, dass die gegebene Erklärung einer weiteren Begründung noch bedarf.

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## ON A NEW KIND OF RAYS

BY

W. C. RÖNTGEN

### FIRST COMMUNICATION

1. If the discharge of a fairly large induction-coil be made to pass through a Hittorf vacuum-tube, or through a Lenard tube, a Crookes tube, or other similar apparatus, which has been sufficiently exhausted, the tube being covered with thin, black card-board which fits it with tolerable closeness, and if the whole apparatus be placed in a completely darkened room, there is observed at each discharge a bright illumination of a paper screen covered with barium platino-cyanide, placed in the vicinity of the induction-coil, the fluorescence thus produced being entirely independent of the fact whether the coated or the plain surface is turned towards the discharge-tube. This fluorescence is visible even when the paper screen is at a distance of two metres from the apparatus.

It is easy to prove that the cause of the fluorescence proceeds from the discharge-apparatus, and not from any other point in the conducting circuit.

2. The most striking feature of this phenomenon is the fact that an active agent here passes through a black card-board envelope, which is opaque to the visible and the ultra-violet rays of the sun or of the electric arc; an agent, too, which has the power of producing active fluorescence. Hence we may first investigate the question whether other bodies also possess this property.

We soon discover that all bodies are transparent to this agent, though in very different degrees. I proceed to give a few examples: Paper is very transparent;* behind a bound book of about one thousand pages I saw the fluorescent screen light up brightly, the printers' ink offering scarcely a noticeable hinderance. In the same way the fluorescence appeared behind a double pack of cards; a single card held between the apparatus and the screen being almost unnoticeable to the eye. A single sheet of tin-foil is also scarcely perceptible; it is only after several layers have been placed over one another that their shadow is distinctly seen on the screen. Thick blocks of wood are also transparent, pine boards two or three centimetres thick absorbing only slightly. A plate of aluminium about fifteen millimetres thick, though it enfeebled the action seriously, did not cause the fluorescence to disappear entirely. Sheets of hard rubber several centimetres thick still permit the rays to pass through them.† Glass plates of equal thickness behave quite differently, according as they contain lead (flint-glass) or not; the former are much less transparent than the latter. If the hand be held between the discharge-tube and the screen, the darker shadow of the bones is seen within the slightly dark shadow-image of the hand itself. Water, carbon disulphide, and various other liquids, when they are

* By "transparency" of a body I denote the relative brightness of a fluorescent screen placed close behind the body, referred to the brightness which the screen shows under the same circumstances, though without the interposition of the body.

† For brevity's sake I shall use the expression "rays"; and to distinguish them from others of this name I shall call them "X-rays." (See Sec. 14.)

examined in mica vessels, seem also to be transparent. That hydrogen is to any considerable degree more transparent than air I have not been able to discover. Behind plates of copper, silver, lead, gold, and platinum the fluorescence may still be recognized, though only if the thickness of the plates is not too great. Platinum of a thickness of 0.2 millimetre is still transparent; the silver and copper plates may even be thicker. Lead of a thickness of 1.5 millimetres is practically opaque; and on account of this property this metal is frequently most useful. A rod of wood with a square cross-section (20×20 millimetres), one of whose sides is painted white with lead paint, behaves differently according as to how it is held between the apparatus and the screen. It is almost entirely without action when the X-rays pass through it parallel to the painted side; whereas the stick throws a dark shadow when the rays are made to traverse it perpendicular to the painted side. In a series similar to that of the metals themselves their salts can be arranged with reference to their transparency, either in the solid form or in solution.

3. The experimental results which have now been given, as well as others, lead to the conclusion that the transparency of different substances, assumed to be of equal thickness, is essentially conditioned upon their density: no other property makes itself felt like this, certainly to so high a degree.

The following experiments show, however, that the density is not the only cause acting. I have examined, with reference to their transparency, plates of glass, aluminium, calcite, and quartz, of nearly the same thickness; and while these substances are almost equal in density, yet it was quite evident that the calcite was sensibly less transparent than the other substances, which appeared almost exactly alike. No particularly strong fluorescence (see Sec. 6) of calcite, especially by comparison with glass, has been noticed.

4. All substances with increase in thickness become less transparent. In order to find a possible relation between transparency and thickness, I have made photographs (see Sec. 6) in which portions of the photographic plate were covered with layers of tin-foil, varying in the number of sheets superposed. Photometric measurements of these will be made when I am in possession of a suitable photometer.

5. Sheets of platinum, lead, zinc, and aluminium were rolled of such thickness that all appeared nearly equally transparent. The following table contains the absolute thickness of these sheets measured in millimetres, the relative thickness referred to that of the platinum sheet, and their densities:

| Thickness | Relative Thickness | Density |
|---|---|---|
| Pt 0.018 mm | 1 | 21.5 |
| Pb 0.05 mm | 3 | 11.3 |
| Zn 0.10 mm | 6 | 7.1 |
| Al 3.5 mm | 200 | 2.6 |

We may conclude from these values that different metals possess transparencies which are by no means equal, even when the product of thickness and density is the same. The transparency increases much more rapidly than this product decreases.

6. The fluorescence of barium platino-cyanide is not the only recognizable effect of the X-rays. It should be mentioned that other bodies also fluoresce; such, for instance, as the phosphorescent calcium compounds, then uranium glass, ordinary glass, calcite, rock-salt, and so on.

Of special significance in many respects is the fact that photographic dry plates are sensitive to the X-rays. We are, therefore, in a condition to determine more definitely many phenomena, and so the more easily to avoid deception; wherever it has been possible, therefore, I have controlled, by means of photography, every important observation which I have made with the eye by means of the fluorescent screen.

In these experiments the property of the rays to pass almost unhindered through thin sheets of wood, paper, and tin-foil is most important. The photographic impressions can be obtained in a non-darkened room with the photographic plates either in the holders or wrapped up in paper. On the other hand, from this property it results as a consequence that undeveloped plates cannot be left for a long time in the neighborhood of the discharge-tube, if they are protected merely by the usual covering of pasteboard and paper.

It appears questionable, however, whether the chemical action on the silver salts of the photographic plates is directly caused by the X-rays. It is possible that this action proceeds from the fluorescent light which, as noted above, is produced in the glass plate itself or perhaps in the layer of gelatin. "Films" can be used just as well as glass plates.

I have not yet been able to prove experimentally that the X-rays are able also to produce a heating action; yet we may well assume that this effect is present, since the capability of the X-rays to be transformed is proved by means of the observed fluorescence phenomena. It is certain, therefore, that all the X-rays which fall upon a substance do not leave it again as such.

The retina of the eye is not sensitive to these rays. Even if the eye is brought close to the discharge-tube, it observes nothing, although, as experiment has proved, the media contained in the eye must be sufficiently transparent to transmit the rays.

7. After I had recognized the transparency of various substances of relatively considerable thickness, I hastened to see how the X-rays behaved on passing through a prism, and to find whether they were thereby deviated or not.

Experiments with water and with carbon disulphide enclosed in mica prisms of about 30° refracting angle showed no deviation, either with the fluorescent screen or on the photographic plate. For purposes of comparison the deviation of rays of ordinary light under the same conditions was observed; and it was noted that in this case the deviated images fell on the plate about 10 or 20 millimetres distant from the direct image. By means of prisms made of hard rubber and of aluminium, also of about 30° refracting angle, I have obtained images on the photographic plate in which some small deviation may perhaps be recognized. However, the fact is quite uncertain; the deviation, if it does exist, being so small that in any case the refractive index of the X-rays in the substances named cannot be more than 1.05 at the most. With a fluorescent screen I was also unable to observe any deviation.

Up to the present time experiments with prisms of denser metals have given no definite results, owing to their feeble transparency and the consequently diminished intensity of the transmitted rays.

With reference to the general conditions here involved on the one hand, and on the other to the importance of the question whether the X-rays can be refracted or not on passing from one medium into another, it is most fortunate that this subject may be investigated in still another way than with the aid of prisms. Finely divided bodies in sufficiently thick layers scatter the incident light and allow only a little of it to pass, owing to reflection and refraction; so that if powders are as transparent to X-rays as the same substances are in mass—equal amounts of material being presupposed—it follows at once that neither refraction nor regular reflection takes place to any sensible degree. Experiments were tried with finely powdered rock-salt, with fine electrolytic silver-powder, and with zinc-dust, such as is used in chemical investigations. In all these cases no difference was detected between the transparency of the powder and that of the substance in mass, either by observation with the fluorescent screen or with the photographic plate.

From what has now been said it is obvious that the X-rays cannot be concentrated by lenses; neither a large lens of hard rubber nor a glass lens having any influence upon them. The shadow-picture of a round rod is darker in the middle than at the edge; while the image of a tube which is filled with a substance more transparent than its own material is lighter at the middle than at the edge.

8. The question as to the reflection of the X-rays may be regarded as settled, by the experiments mentioned in the preceding paragraph, in favor of the view that no noticeable regular reflection of the rays takes place from any of the substances examined. Other experiments, which I here omit, lead to the same conclusion.

One observation in this connection should, however, be mentioned, as at first sight it seems to prove the opposite. I exposed to the X-rays a photographic plate which was protected from the light by black paper, and the glass side of which was turned towards the discharge-tube giving the X-rays. The sensitive film was covered, for the most part, with polished plates of platinum, lead, zinc, and aluminium arranged in the form of a star. On the developed negative it was seen plainly that the darkening under the platinum, the lead, and particularly the zinc, was stronger than under the other plates, the aluminium having exerted no action at all. It appears, therefore, that these three metals reflect the rays. Since, however, other explanations of the stronger darkening are conceivable, in a second experiment, in order to be sure, I placed between the sensitive film and the metal plates a piece of thin aluminium-foil, which is opaque to ultra-violet rays, but is very transparent to the X-rays. Since the same result substantially was again obtained, the reflection of X-rays from the metals above named is proved.

If we compare this fact with the observation already mentioned that powders are as transparent as coherent masses, and with the further fact that bodies with rough surfaces behave like polished bodies with reference to the passage of the X-rays, as shown also in the last experiment, we are led to the conclusion already stated that regular reflection does not take place, but that bodies behave towards the X-rays as turbid media do towards light.

Since, moreover, I could detect no evidence of refraction of these rays in passing from one medium into another, it would seem that X-rays move with the same velocity in all substances; and, further, that this speed is the same in the medium which is present everywhere in space and in which the particles of matter are imbedded. These particles hinder the propagation of the X-rays, the effect being greater, in general, the more dense the substance concerned.

9. Accordingly it might be possible that the arrangement of particles in the substance exercised an influence on its transparency; that, for instance, a piece of calcite might be transparent in different degrees for the same thickness, according as it is traversed in the direction of the axis, or at right angles to it. Experiments, however, on calcite and quartz gave a negative result.

10. It is well known that Lenard came to the conclusion, from the results of his beautiful experiments on the transmission of the cathode rays of Hittorf through a thin sheet of aluminium, that these rays are phenomena of the ether, and that they diffuse themselves through all bodies. We can say the same of our rays.

In his most recent research, Lenard has determined the absorptive power of different substances for the cathode rays, and, among others, has measured it for air from atmospheric pressure to 4.10, 3.40, 3.10, referred to 1 centimetre, according to the rarefaction of the gas contained in the discharge-apparatus. Judging from the discharge-pressure as estimated from the sparking distance, I have had to do in my experiments for the most part with rarefactions of the same order of magnitude, and only rarely with less or greater ones. I have succeeded in comparing by means of the L. Weber photometer—I do not possess a better one—the intensities, taken in atmospheric air, of the fluorescence of my screen at two distances from the discharge-apparatus—about 100 and 200 millimetres; and I have found from three experiments, which agree very well with each other, that the intensities vary inversely as the squares of the distances of the screen from the discharge-apparatus. Accordingly, air absorbs a far smaller fraction of the X-rays than of the cathode rays. This result is in entire agreement with the observation mentioned above, that it is still possible to detect the fluorescent light at a distance of 2 metres from the discharge-apparatus.

Other substances behave in general like air; they are more transparent to X-rays than to cathode rays.

11. A further difference, and a most important one, between the behavior of cathode rays and of X-rays lies in the fact that I have not succeeded, in spite of many attempts, in obtaining a deflection of the X-rays by a magnet, even in very intense fields.

The possibility of deflection by a magnet has, up to the present time, served as a characteristic property of the cathode rays; although it was observed by Hertz and Lenard that there are different sorts of cathode rays, "which are distinguished from each other by their production of phosphorescence, by the amount of their ab-

sorption, and by the extent of their deflection by a magnet." A considerable deflection, however, was noted in all of the cases investigated by them; so that I do not think that this characteristic will be given up except for stringent reasons.

12. According to experiments especially designed to test the question, it is certain that the spot on the wall of the discharge-tube which fluoresces the strongest is to be considered as the main centre from which the X-rays radiate in all directions. The X-rays proceed from that spot where, according to the data obtained by different investigators, the cathode rays strike the glass wall. If the cathode rays within the discharge-apparatus are deflected by means of a magnet, it is observed that the X-rays proceed from another spot—namely, from that which is the new terminus of the cathode rays.

For this reason, therefore, the X-rays, which it is impossible to deflect, cannot be cathode rays simply transmitted or reflected without change by the glass wall. The greater density of the gas outside of the discharge-tube certainly cannot account for the great difference in the deflection, according to Lenard.

I therefore reach the conclusion that the X-rays are not identical with the cathode rays, but that they are produced by the cathode rays at the glass wall of the discharge-apparatus.

13. This production does not take place in glass alone, but, as I have been able to observe in an apparatus closed by a plate of aluminium 2 millimetres thick, in this metal also. Other substances are to be examined later.

14. The justification for calling by the name "rays" the agent which proceeds from the wall of the discharge-apparatus I derive in part from the entirely regular formation of shadows, which are seen when more or less transparent bodies are brought between the apparatus and the fluorescent screen (or the photographic plate).

I have observed, and in part photographed, many shadow-pictures of this kind, the production of which has a particular charm. I possess, for instance, photographs of the shadow of the profile of a door which separates the rooms in which, on one side, the discharge-apparatus was placed, on the other the photographic plate; the shadow of the bones of the hand; the shadow of a covered wire wrapped on a wooden spool; of a set of weights enclosed in a box; of a galvanometer in which the magnetic needle is entirely enclosed by metal; of a piece of metal whose lack of homogeneity becomes noticeable by means of the X-rays, etc.

Another conclusive proof of the rectilinear propagation of the X-rays is a pin-hole photograph which I was able to make of the discharge-apparatus while it was enveloped in black paper; the picture is weak but unmistakably correct.

15. I have tried in many ways to detect interference phenomena of the X-rays; but, unfortunately, without success, perhaps only because of their feeble intensity.

16. Experiments have been begun, but are not yet finished, to ascertain whether electrostatic forces affect the X-rays in any way.

17. In considering the question what are the X-rays—which, as we have seen, cannot be cathode rays—we may perhaps at first be led to think of them as ultra-violet light, owing to their active fluorescence and their chemical actions. But in so doing we find ourselves opposed by the most weighty considerations. If the X-rays are ultra-violet light, this light must have the following properties:

(*a*) On passing from air into water, carbon disulphide, aluminium, rock-salt, glass, zinc, etc., it suffers no noticeable refraction.

(*b*) By none of the bodies named can it be regularly reflected to any appreciable extent.

(*c*) It cannot be polarized by any of the ordinary methods.

(*d*) Its absorption is influenced by no other property of substances so much as by their density.

That is to say, we must assume that these ultra-violet rays behave entirely differently from the ultra-red, visible, and ultra-violet rays which have been known up to this time.

I have been unable to come to this conclusion, and so have sought for another explanation.

There seems to exist some kind of relationship between the new rays and light rays; at least this is indicated by the formation of shadows, the fluorescence and the chemical action produced by them both. Now, we have known for a long time that there can be in the ether longitudinal vibrations besides the transverse light-vibrations; and, according to the views of different physicists, these vibrations must exist. Their existence, it is true, has not been proved up to the present, and consequently their properties have not been investigated by experiment.

Ought not, therefore, the new rays to be ascribed to longitudinal vibrations in the ether?

I must confess that in the course of the investigation I have become more and more confident of the correctness of this idea, and so, therefore, permit myself to announce this conjecture, although I am perfectly aware that the explanation given still needs further confirmation.

WÜRZBURG, Physikalisches Institut der Universität.
*December, 1895.*

### SECOND COMMUNICATION

Since my work must be interrupted for several weeks, I take the opportunity of presenting in the following paper some new phenomena which I have observed.

18. It was known to me at the time of my first publication that X-rays can discharge electrified bodies; and I conjecture that in Lenard's experiments it was the X-rays, and not the cathode rays, which had passed unchanged through the aluminium window of his apparatus, which produced the action described by him upon electrified bodies at a distance. I have, however, delayed the publication of my experiments until I could contribute results which are free from criticism.

These results can be obtained only when the observations are made in a space which is protected completely, not only from the electrostatic forces proceeding from the vacuum-tube, from the conducting wires, from the induction apparatus, etc., but is also closed against air which comes from the neighborhood of the discharge-apparatus.

To secure these conditions I had a chamber made of zinc

Röntgen: Ueber eine neue Art von Strahlen. (II. Mittheilung.) 11

Genehmigung und es werden der I. und der II. Vorstand dem Jubilar gratuliren.

4. Herr Röntgen, von lebhaftem, langanhaltendem Beifall begrüsst, hält seinen angekündigten Vortrag über: „Eine neue Art von Strahlen"*). Gegen Schluss desselben wird nach dem neuen Verfahren der Schattenrisse des Skelettes einer menschlichen Hand photographisch aufgenommen und zwar der Rechten des Ehrenpräsidenten der Gesellschaft, Herrn v. Kölliker. Letzterer dankt im Namen der Gesellschaft dem Vortragenden für die Mittheilungen, die in den Annalen der Sitzungen an Bedeutung ihres Gleichen nicht haben, und bringt auf Herrn Röntgen ein Hoch aus, in welches die Mitglieder und das gesammte, den Hörsaal des physikalischen Instituts gedrängt füllende Auditorium dreimal mit lautem Ruf und unter rauschendem Beifall einstimmen. Der Vorschlag Herrn v. Kölliker's, die neuen „X-Strahlen" von nun an „Röntgensche Strahlen" zu nennen, entfesselt neuen allgemeinen Jubelruf.

In der vom I. Vorsitzenden eingeleiteten Discussion sprechen die Herren v. Kölliker und Röntgen über die Möglichkeit, die neuen Strahlen für medicinische Zwecke dienstbar zu machen.**)

Der I. Vorsitzende schliesst hierauf die hochbedeutsame Sitzung, indem er noch dem Vortragenden seinen ganz besonderen Dank dafür ausspricht, dass er zur ersten Veröffentlichung seiner Untersuchungen das Organ der physikalisch-medicinischen Gesellschaft gewählt hat.

## W. C. Röntgen: Ueber eine neue Art von Strahlen.

### II. Mittheilung.

(Als Beitrag eingereicht.)

Da meine Arbeit auf mehrere Wochen unterbrochen werden muss, gestatte ich mir im Folgenden einige neue Ergebnisse schon jetzt mitzutheilen.

18. Zur Zeit meiner ersten Publication war mir bekannt, dass die X-Strahlen im Stande sind, electrische Körper zu entladen.

*) cf. Sitzungsberichte 1895, pag. 132.

**) *Herr v. Kölliker* bemerkt, dass die neue Entdeckung voraussichtlich auch eine grosse Bedeutung auf medicinischem Gebiet haben werde: Gelegenheit, die X-Strahlen zur Durchleuchtung Kranker zu verwenden, sei ja an dem reichen Material der hiesigen Kliniken geboten und eine Unterstützung der Mediciner dabei durch Herrn *Röntgen* wohl zu hoffen. Es scheinen wohl fürs Erste chirurgische Affectionen, vor Allem Veränderungen am Knochengerüst der Exploration durch die neuen Strahlen zugänglich zu sein.

Herr *Röntgen* erwidert, dass zum Durchleuchten von Körpertheilen, die wesentlich dicker sind als Arme und Beine, intensivere Röhren als die bisherigen construirt werden müssen und dass er mit dieser Aufgabe beschäftigt ist. Welche inneren Theile des menschlichen Körpers mit den verbesserten Röhren sichtbar gemacht werden können, lässt sich zur Zeit nicht sagen: das hängt von dem Grade ihrer noch nicht untersuchten Durchlässigkeit und von ihrer Lage im Körper ab.

12 Sitzungsberichte der physikal.-medicin. Gesellschaft. Jahrg. 1896.

und ich vermuthe, dass es auch die X-Strahlen und nicht die von dem Aluminiumfenster seines Apparates unverändert durchgelassenen Kathodenstrahlen gewesen sind, welche die von *Lenard* beschriebene Wirkung auf entfernte electrische Körper ausgeübt haben. Mit der Veröffentlichung meiner Versuche habe ich aber gewartet, bis ich in der Lage war, einwurfsfreie Resultate mitzutheilen.

Solche lassen sich wohl nur dann erhalten, wenn man die Beobachtungen in einem Raum anstellt, der nicht nur vollständig gegen die von der Vacuumröhre, den Zuleitungsdrähten, dem Inductionsapparat etc. ausgehenden electrostatischen Kräfte geschützt ist, sondern der auch gegen Luft abgeschlossen ist, welche aus der Nähe des Entladungsapparates kommt.

Ich liess mir zu diesem Zweck aus zusammengelötheten Zinkblechen einen Kasten anfertigen, der gross genug ist, um mich und die nöthigen Apparate aufzunehmen, und der bis auf ein durch eine Zinkthüre verschliessbare Oeffnung überall luftdicht verschlossen ist. Die der Thüre gegenüber liegende Wand ist zu einem grossen Theil mit Blei belegt; an einer dem ausserhalb des Kastens aufgestellten Entladungsapparat nahe gelegenen Stelle wurde die Zinkwand mit der darüber gelegten Bleiplatte in einer Weite von 4 cm ausgeschnitten, und die Oeffnung ist mit einem dünnen Aluminiumblech wieder luftdicht verschlossen. Durch dieses Fenster können die X-Strahlen in den Beobachtungskasten eindringen.

Ich habe nun Folgendes wahrgenommen:

a) In der Luft aufgestellte, positiv oder negativ electrisch geladene Körper werden, wenn sie mit X-Strahlen bestrahlt werden, entladen und zwar desto rascher, je intensiver die Strahlen sind. Die Intensität der Strahlen wurde nach ihrer Wirkung auf einen Fluorescenzschirm oder auf eine photographische Platte beurtheilt.

Es ist im Allgemeinen gleichgültig, ob die electrischen Körper Leiter oder Isolatoren sind. Bis jetzt habe ich auch keinen specifischen Unterschied in dem Verhalten der verschiedenen Körper bezüglich der Geschwindigkeit der Entladung gefunden; ebensowenig in dem Verhalten von positiver und negativer Electricität. Doch ist es nicht ausgeschlossen, dass geringe Unterschiede bestehen.

Josef Albert, München, repr.

**Hand des Anatomen Geheimrath von Kölliker**

Im Physikal. Institut der Universität Würzburg
mit X-Strahlen aufgenommen
von Professor Dr. W. C. Röntgen

Röntgen: Ueber eine neue Art von Strahlen. (II. Mittheilung). 15

b) Ist ein electrisirter Leiter nicht von Luft sondern von einem festen Isolator z. B. Paraffin umgeben, so bewirkt die Bestrahlung dasselbe, wie das Bestreichen der isolirenden Hülle mit einer zur Erde abgeleiteten Flamme.

c) Ist diese isolirende Hülle von einem eng anliegenden, zur Erde abgeleiteten Leiter umschlossen, welcher wie der Isolator für X-Strahlen durchlässig sein soll, so übt die Bestrahlung auf den inneren, electrisirten Leiter keine mit meinen Hülfsmitteln nachweisbare Wirkung aus.

d) Die unter a, b, c mitgetheilten Beobachtungen deuten darauf hin, dass die von den X-Strahlen bestrahlte Luft die Eigenschaft erhalten hat, electrische Körper, mit denen sie in Berührung kommt, zu entladen.

e) Wenn sich die Sache wirklich so verhält, und wenn ausserdem die Luft diese Eigenschaft noch einige Zeit behält, nachdem sie den X-Strahlen ausgesetzt war, so muss es möglich sein, electrische Körper, welche selbst nicht von den X-Strahlen getroffen werden, dadurch zu entladen, dass man ihnen bestrahlte Luft zuführt.

In verschiedener Weise kann man sich davon überzeugen, dass diese Folgerung in der That zutrifft. Eine, wenn auch nicht die einfachste, Versuchsanordnung möchte ich mittheilen.

Ich benutzte eine 3 cm weite, 45 cm lange Messingröhre; in einigen Centimeter Entfernung von dem einen Ende ist ein Theil der Röhrenwand weggeschnitten und durch ein dünnes Aluminiumblech ersetzt; am anderen Ende ist unter luftdichtem Abschluss eine an einer Metallstange befestigte Messingkugel isolirt in die Röhre eingeführt. Zwischen der Kugel und dem verschlossenen Ende der Röhre ist ein Seitenröhrchen angelöthet, das mit einer Saugvorrichtung in Verbindung gesetzt werden kann; wenn gesaugt wird, so wird die Messingkugel umspült von Luft, die auf ihrem Wege durch die Röhre an dem Aluminiumfenster vorüber gegangen ist. Die Entfernung vom Fenster bis zur Kugel beträgt über 20 cm.

Diese Röhre stellte ich im Zinkkasten so auf, dass die X-Strahlen durch das Aluminiumfenster der Röhre, senkrecht zur Axe derselben eintreten konnten, die isolirte Kugel lag dann ausserhalb des Bereiches dieser Strahlen, im Schatten. Die Röhre

und der Zinkkasten waren leitend mit einander, die Kugel mit einem Hankel'schen Electroskop verbunden.

Es zeigte sich nun, dass eine der Kugel mitgetheilte Ladung (positive oder negative) von den X-Strahlen nicht beeinflusst wurde, so lange die Luft in der Röhre in Ruhe blieb, dass die Ladung aber sofort beträchtlich abnahm, wenn durch kräftiges Saugen bestrahlte Luft der Kugel zugeführt wurde. Er hielt die Kugel durch Verbindung mit Accumulatoren ein constantes Potential, und wurde fortwährend bestrahlte Luft durch die Röhre gesaugt, so entstand ein electrischer Strom, wie wenn die Kugel mit der Röhrenwand durch einen schlechten Leiter verbunden gewesen wäre.

f) Es fragt sich, in welcher Weise die Luft die ihr von den X-Strahlen mitgetheilte Eigenschaft wieder verlieren kann. Ob sie sie von selbst, d. h. ohne mit anderen Körpern in Berührung zu kommen, mit der Zeit verliert, ist noch unentschieden. Sicher dagegen ist es, dass eine kurz dauernde Berührung mit einem Körper von grosser Oberfläche, der nicht electrisch zu sein braucht, die Luft unwirksam machen kann. Schiebt man z. B. einen genügend dicken Pfropf aus Watte in die Röhre so weit ein, dass die bestrahlte Luft die Watte durchstreichen muss, bevor sie zu der electrischen Kugel gelangt, so bleibt die Ladung der Kugel auch beim Saugen unverändert.

Sitzt der Pfropf an einer Stelle, die vor dem Aluminiumfenster liegt, so erhält man dasselbe Resultat wie ohne Watte: ein Beweis, dass nicht etwa Staubtheilchen die Ursache der beobachteten Entladung sind.

Drahtgitter wirken ähnlich wie Watte; doch muss das Gitter sehr eng sein, und viele Lagen müssen über einander gelegt werden, wenn die durchgestrichene, bestrahlte Luft unwirksam sein soll. Sind diese Gitter nicht, wie bisher angenommen, zur Erde abgeleitet, sondern mit einer Electricitätsquelle von constantem Potential verbunden, so habe ich immer das beobachtet, was ich erwartet hatte: doch sind diese Versuche noch nicht abgeschlossen

(Fortsetzung folgt.)


# Sitzungs-Berichte

der

## Physikalisch-medicinischen Gesellschaft

zu

## WÜRZBURG.

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### W. C. Röntgen: Ueber eine neue Art von Strahlen.

II. Mittheilung.

(Fortsetzung u. Schluss.)

g) Befinden sich die electrischen Körper statt in Luft in trocknem Wasserstoff, so werden sie ebenfalls durch die X-Strahlen entladen. Die Entladung in Wasserstoff schien mir etwas langsamer zu verlaufen, doch ist diese Angabe noch unsicher wegen der Schwierigkeit, bei aufeinander folgenden Versuchen gleiche Intensität der X-Strahlen zu erhalten.

Die Art und Weise der Füllung der Apparate mit Wasserstoff dürfte die Möglichkeit ausschliessen, dass die anfänglich auf der Oberfläche der Körper vorhandene verdichtete Luftschicht bei der Entladung eine wesentliche Rolle gespielt hätte.

h) In stark evacuirten Räumen findet die Entladung eines direct von den X-Strahlen getroffenen Körpers viel langsamer — in einem Fall z. B. ca. 70mal langsamer — statt, als in denselben Gefässen, welche mit Luft oder Wasserstoff von Atmosphärendruck gefüllt sind.

i) Versuche über das Verhalten einer Mischung von Chlor und Wasserstoff unter dem Einfluss der X-Strahlen sind in Angriff genommen

2



j) Schliesslich möchte ich noch erwähnen, dass die Resultate von Untersuchungen über die entladende Wirkung der X-Strahlen, bei welchen der Einfluss des umgebenden Gases unberücksichtigt blieb, vielfach mit Vorsicht aufzunehmen sind.

19. In manchen Fällen ist es vortheilhaft, zwischen den die X-Strahlen liefernden Entladungsapparat und den Ruhmkorff einen Tesla'schen Apparat (Condensator und Transformator) einzuschalten. Diese Anordnung hat folgende Vorzüge: erstens werden die Entladungsapparate weniger leicht durchschlagen und weniger warm, zweitens hält sich das Vacuum, wenigstens bei meinen selbstangefertigten Apparaten, längere Zeit, und drittens liefern manche Apparate intensivere X-Strahlen Bei Apparaten, die zu wenig oder zu stark evacuirt waren, um mit dem Ruhmkorff allein gut zu functioniren, leistete die Anwendung des Tesla'schen Transformators gute Dienste.

Es liegt die Frage nahe – und ich gestatte mir deshalb sie zu erwähnen, ohne zu ihrer Beantwortung vorläufig etwas beitragen zu können — ob auch durch eine continuirliche Entladung mit constant bleibendem Entladungspotential X-Strahlen erzeugt werden können; oder ob nicht vielmehr Schwankungen dieses Potentials zum Entstehen derselben durchaus erforderlich sind.

20. In § 13 meiner ersten Veröffentlichung ist mitgetheilt, dass die X-Strahlen nicht blos in Glas sondern auch in Aluminium entstehen können. Bei der Fortsetzung der Untersuchung nach dieser Richtung hin hat sich kein fester Körper ergeben, welcher nicht im Stande wäre, unter dem Einflusse der Kathodenstrahlen X-Strahlen zu erzeugen. Es ist mir auch kein Grund bekannt geworden, weshalb sich flüssige und gasförmige Körper nicht ebenso verhalten würden.

Quantitative Unterschiede in dem Verhalten der verschiedenen Körper haben sich dagegen ergeben. Lässt man z. B. die Kathodenstrahlen auf eine Platte fallen, deren eine Hälfte aus einem 0,3 mm dicken Platinblech, deren andere Hälfte aus einem 1 mm dicken Aluminiumblech besteht, so beobachtet man an dem mit der Lochcamera aufgenommenen photographischen Bild dieser Doppelplatte, dass das Platinblech auf der von den Kathodenstrahlen getroffenen (Vorder-)Seite viel mehr X-Strahlen aussendet, als das Aluminiumblech auf der gleichen Seite. Von der



Hinterseite dagegen gehen vom Platin so gut wie gar keine, vom Aluminium aber relativ viel X-Strahlen aus. Letztere Strahlen sind in den vorderen Schichten des Aluminiums erzeugt und durch die Platte hindurch gegangen.

Man kann sich von dieser Beobachtung leicht eine Erklärung verschaffen, doch dürfte es sich empfehlen, vorher noch weitere Eigenschaften der X-Strahlen zu erfahren.

Zu erwähnen ist aber, dass der gefundenen Thatsache auch eine praktische Bedeutung zukommt. Zur Erzeugung von möglichst intensiven X-Strahlen eignet sich nach meinen bisherigen Erfahrungen Platin am besten. Ich gebrauche seit einigen Wochen mit gutem Erfolg einen Entladungsapparat, bei dem ein Hohlspiegel aus Aluminium als Kathode, ein unter 45° gegen die Spiegelaxe geneigtes, im Krümmungscentrum aufgestelltes Platinblech als Anode fungirt.

21. Die X-Strahlen gehen bei diesem Apparat von der Anode aus. Wie ich aus Versuchen mit verschieden geformten Apparaten schliessen muss, ist es mit Rücksicht auf die Intensität der X-Strahlen gleichgültig, ob die Stelle, wo diese Strahlen erzeugt werden, die Anode ist oder nicht.

Speciell zu den Versuchen mit den Wechselströmen des Tesla'schen Transformators wird ein Entladungsapparat angefertigt, bei dem beide Electroden Aluminiumhohlspiegel sind, deren Axen mit einander einen rechten Winkel bilden; im gemeinschaftlichen Krümmungscentrum ist eine die Kathodenstrahlen auffangende Platinplatte angebracht. Ueber die Brauchbarkeit dieses Apparates soll später berichtet werden

Abgeschlossen: 9. März 1896

Würzburg. Physikal. Institut d. Universität.

## IV. Sitzung vom 6. Februar 1896.

1. Herr Dr. Paul Hauptfleisch, Privatdocent und Assistent am botanischen Institut, wird als ordentliches Mitglied in die Gesellschaft aufgenommen.
2. Das Protokoll der 2ten und 3ten Sitzung wird verlesen und genehmigt.
3. Durch Herrn A. Fick wird Herr Dr Andreas Michel, Zahnarzt in Würzburg, zur Aufnahme als ordentliches Mitglied der Gesellschaft vorgeschlagen.

2*

plates soldered together, which was large enough to contain myself and the necessary apparatus, which could be closed airtight, and which was provided with an opening which could be closed by a zinc door. The wall opposite the door was for the most part covered with lead. At a place near the discharge-apparatus, which was set up outside the case, the zinc wall, together with the lining of sheet-lead, was cut out for a width of 4 centimetres; and the opening was covered again air-tight with a thin sheet of aluminium. The X-rays penetrated through this window into the observation space.

I observed the following phenomena:

(*a*) Electrified bodies in air, charged either positively or negatively, are discharged if X-rays fall upon them; and this process goes on the more rapidly the more intense the rays are. The intensity of the rays was estimated by their action on a fluorescent screen or a photographic plate.

It is immaterial in general whether the electrified bodies are conductors or insulators. Up to the present I have not found any specific difference in the behavior of different bodies with reference to the rate of discharge; nor as to the behavior of positive and negative electricity. Yet it is not impossible that small differences may exist.

(*b*) If the electrified conductor be surrounded not by air but by a solid insulator, *e.g.* paraffin, the radiation has the same action as would result from exposure of the insulating envelope to a flame connected to the earth.

(*c*) If this insulating envelope be surrounded by a close-fitting conductor which is connected to the earth, and which, like the insulator, is transparent to X-rays, the radiation produces on the inner electrified conductor no action which can be detected by my apparatus.

(*d*) The observations noted under (*a*), (*b*), (*c*) indicate that air through which X-rays have passed possesses the power of discharging electrified bodies with which it comes in contact.

(*e*) If this is really the case, and if, further, the air retains this property for some time after it has been exposed to the X-rays, then it must be possible to discharge electrified bodies which have not been themselves exposed to the rays, by conducting to them air which has thus been exposed.

We may convince ourselves in various ways that this conclusion is correct. One method of experiment, although perhaps not the simplest, I shall describe.

I used a brass tube 3 centimetres wide and 45 centimetres long; at a distance of some centimetres from one end a part of the wall of the tube was cut away and replaced by a thin aluminium plate; at the other end, through an air-tight cap, a brass ball fastened to a metal rod was introduced into the tube in such a manner as to be insulated. Between the ball and the closed end of the tube there was soldered a side-tube which could be connected with an exhaust-apparatus; so that when this is in action the brass ball is subjected to a stream of air which on its way through the tube has passed by the aluminium window. The distance from the window to the ball was over 20 centimetres.

I arranged this tube inside the zinc chamber in such a position that the X-rays could enter through the aluminium window of the tube perpendicular to its axis. The insulated ball lay then in the shadow, out of the range of the action of these rays. The tube and the zinc case were connected by a conductor, the ball was joined to a Hankel electroscope.

It was now observed that a charge (either positive or negative) given to the ball was not influenced by the X-rays so long as the air remained at rest in the tube, but that the charge instantly decreased considerably if by exhaustion the air which had been subjected to the rays was drawn past the ball. If by means of storage cells the ball was maintained at a constant potential, and if the modified air was drawn continuously through the tube, an electric current arose just as if the ball were connected to the wall of the tube by a poor conductor.

(*f*) The question arises, How does the air lose the property which is given it by the X-rays? It is not yet settled whether it loses this property gradually of itself—i.e., without coming in contact with other bodies. On the other hand, it is certain that a brief contact with a body of large surface, which does not need to be electrified, can make the air inactive. For instance, if a thick enough stopper of wadding is pushed into the tube so far that the modified air must pass through it before it reaches the electrified ball, the charge on the ball remains unaffected even while the exhaustion is taking place.

If the wad is in front of the aluminium window, the result obtained is the same as it would be without the wad; a proof that it is not particles of dust which are the cause of the observed discharge.

Wire gratings act like wadding; but the gratings must be very fine, and many layers must be placed over each other if the modified air is to be inactive after it is drawn through them. If these gratings are not connected to the earth, as has been assumed, but are connected to a source of electricity at a constant potential, I have always observed exactly what I had expected; but these experiments are not yet completed.

(*g*) If the electrified bodies, instead of being in air, are placed in dry hydrogen, they are also discharged by the X-rays. The discharge in hydrogen seemed to me to proceed somewhat more slowly; yet this is still uncertain on account of the difficulty of obtaining exactly equal intensities of the X-rays in consecutive experiments.

The method of filling the apparatus with hydrogen precludes the possibility that the layer of air which was originally present, condensed on the surface of the bodies, played any important rôle.

(*h*) In spaces which are highly exhausted the discharge of a body by the direct incidence of X-rays proceeds much more slowly—in one case about seventy times more slowly—than in the same vessels when filled with air or hydrogen at atmospheric pressure.

(*i*) Experiments are about to be begun on the behavior of a mixture of chlorine and hydrogen under the influence of X-rays.

(*j*) In conclusion I would like to mention that the results of investigations on the discharging action of X-rays in which the influence of the surrounding gas is not taken into account should be received with great caution.

19. It is advantageous in many cases to include a Tesla apparatus (condenser and transformer) between the discharge-apparatus which furnishes the X-rays and the induction-coil. This arrangement has the following advan-

tages: first, the discharge-apparatus is less easily penetrated and is less heated; second, the vacuum maintains itself for a longer time, at least in my self-constructed apparatus; third, many discharge-tubes under these conditions give more intense X-rays. With tubes which have not been exhausted sufficiently or have been exhausted too much to be driven satisfactorily by the induction-coil alone, the addition of the Tesla transformer renders good service.

The question immediately arises—and I allow myself to mention it without being able to contribute anything to its solution at present—whether X-rays can be produced by a continuous discharge under constant difference of potential; or whether variations of this potential are essential and necessary for the production of the rays.

20. In paragraph 13 of my first memoir I announced that X-rays could originate not only in glass, but in aluminium also. In the continuation of my experiments in this direction I have not found any solid body which cannot, under the action of the cathode rays, produce X-rays. There is also no reason known to me why liquids and gases may not behave in the same manner.

Quantitative differences in the behavior of different substances have appeared, however. If, for instance, the cathode rays fall upon a plate one half of which is made of platinum 0.3 millimetre thick, the other half of aluminium 1 millimetre thick, we see on the photographic image of this double plate, taken by means of a pin-hole camera, that the platinum sends out many more X-rays from the side struck by the cathode rays (the front side) than does the aluminium from the same side. However, from the rear side the platinum emits practically no X-rays, while the aluminium sends out relatively many. These last rays are produced in the front layers of the aluminium and pass through the plate.

We can easily devise an explanation of this observation, yet it may be advisable to learn other properties of the X-rays before so doing.

It must be mentioned, however, that there is a practical importance in the facts observed. For the production of the most intense X-rays platinum is best suited, according to my experiments up to the present. I have used for some weeks with great success a discharge-apparatus in which the cathode is a concave mirror of aluminium, and the anode is a plate of platinum placed at the centre of curvature of the mirror and inclined to the axis of the mirror at an angle of 45°.

21. The X-rays proceed in this case from the anode. I must conclude, though, from experiments with apparatus of different kinds that it is entirely immaterial, so far as the intensity of the X-rays is concerned, whether the place where the rays are produced is the anode or not.

A discharge-apparatus was prepared specially for experiments with the alternating currents of the Tesla transformer; in it both electrodes were aluminium concave mirrors whose axes were at right angles; at their common centre of curvature there was placed a platinum plate to receive the cathode rays. Further information will be given later as to the usefulness of this apparatus.

WÜRZBURG, Physikalisches Institut der Universität.
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# Physics and the Need for Creative Experience in General Education

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THE rapid growth of physics as a profession, with its war research record and its expansion in industrial research, produces some ill-advised optimism on the part of teachers of college physics. No doubt after the war a larger number of able minds will be attracted to physics as a career. But why should teachers of college physics expect an increased enrolment of students interested in a general education? The success of physics in war research will be to prospective students a matter of hearsay rather than of familiar interest. Moreover, experience shows that applications of physics in transportation, communication and home conveniences, though numerous, have not made physics popular. Instead of building up false hopes, it is greater wisdom for the teacher of college physics to seek through new endeavor to make the college course in physics contribute more than it does now to the value of general education.

College students are interested in the use of the mind. They appreciate an intellectual thrill. The challenge to teachers of physics continues, and the opportunity for additional effort is ample. If we can resist tradition, many ways will be found in which the study of physics can be made more valuable. The discussion that follows will indicate clearly, it is hoped, one direction in which college physics can improve its contribution to general education. The claim will be made that physics has a definite advantage over many other subjects in its ability to give an opportunity for experience in creativeness on the part of the ordinary student.

One of the greatest weaknesses in higher education results from the absence of plans for the exercise of creativeness by college students. A college graduate does not have the opportunity to acquire confidence in his ability to do anything outside of the beaten path. Every teacher of

graduate work knows that this is true of even those graduates who are highly selected. Yet if creativeness is described as an act of combining two or more components in such a way that there results something new to the individual, evidently there is a definite element of creativeness in the activities of everyone, each in his own walk of life and at his own level of attainment. Thus there ought to be no question but that the exercise of the creativeness of the ordinary man is an important part of his training.

What is the usual view of creativeness acquired by a student in college, as compared with that obtained subsequently through experience as a graduate student? If the graduate view is more nearly correct, why should it not have been adopted earlier? In the undergraduate course of study the student gave some attention to English literature and perhaps to other fine arts. There his mind was impressed with the evidence of gifts far above his own level of talent, this being but a continuation of earlier experiences. The masters in literature and in the other fine arts gave him his first clear view of creativeness. It appeared to be a gift possessed by relatively few persons. According to that standard there could be no appreciable creativeness in the ordinary man. But this is incorrect. An essayist has claimed that perhaps only once in 75 years on the average does there arise a genius in English literature. Contrast this with the conservative estimate that there are at present 10,000 persons in our own country earning a living by writing. Rather obviously their writing must have an element of creativeness to be salable. The view of the scarcity of creative ability usually acquired by the undergraduate is not in accordance with the facts, even in the art of writing.

In discussing the influence of graduate study it is simpler to take a specific case such as that of the young physicist. In his graduate work he gives close study to the work of great men and women, and there he sees that these distinguished individuals made use chiefly of well-known attitudes and abilities—initiative, persistence in thinking, deep boring, unlimited patience, ability to analyze, and alertness of mind. They were motivated in part by the continued conviction that there was always work to be done. The physics student observes that these great people all had a high order of intelligence, but, more importantly, in each individual there occurred a conjunction of good qualities of mind and a strong, active, fighting spirit. To our graduate student, what is called a stroke of genius in physics loses much of its mystery. He begins to believe that any able student should be able to do creative work of at least a minor character even in a difficult subject like physics. So the view of the student is altered by a more intimate knowledge of physics.

What further change in view is subsequently acquired by the young physicist? Through contact with successful people in other walks of life, his view of creativeness expands yet farther. Here experience shows that in all careers there is the same chance to face problems with intelligence, initiative and persistence. The physicist recognizes that the lawyer may suggest to his client new and more economical methods of attaining given ends, that the doctor must be at home in a case much different from the standardized textbook one, that the carpenter ingeniously improves upon the original working drawings, and that the laborer applies novel short cuts in unusual conditions. He notices that these people are successful because they are creative. With this experience, the physicist is now impressed with the fact that the opportunity for creativeness in the world's work exists with everyone and is important to success, to happiness and to economic security. Everyone has the chance to be creative, each at his own level of attainment.

Elementary education has in the past 30 years demonstrated that there is a surprising amount of creative potentiality in children. This prospecting has been limited practically to the fine arts because that has been the direction which did not require much preparation in knowledge for the child. In higher education, where the courses of study approach nearer to usefulness in chosen careers, there ought to be possible a great deal of opportunity to provide students with experience in creativeness. Unfortunately this has not been realized. Higher education has not adopted experience in creativeness as one of its goals. In college the teaching is done by the specialist who is absorbed by the necessity of giving the student as much subject matter as possible within the time allotted. It is correctly assumed that the ordinary college student will not exhibit sufficient originality and initiative to lead to new knowledge or new interpretations. The time for that activity, reasons the professor, should come only after the student is fully prepared with information, or in graduate work. Everyone would agree with this opinion if by its adoption the student's future were not

hampered by the lack of frequent contact with practice in creativeness. Creativeness is natural, but it is a function that can atrophy. It is not easily activated after having been repressed over a long period during the individual's mental development. The effects of disuse and the difficulty of subsequent stimulation are constantly observed by teachers who direct beginning graduate students. The habit of complete dependence upon books and the conventional learning process shuts out any interest in inquiry. When the inquiring mind ceases habitual operation, there is no creativeness possible. The student's nature requires experience in creativeness even though it be, in a sense, a practice performance. Every game of skill requires an enormous amount of preliminary practice wherein there is no addition to knowledge. It is necessary that the student actually indulge in practice, receiving mental satisfaction and stimulus, and developing confidence in his own creative powers. Yet higher education has neglected this evidently desirable function.

This neglect on the part of higher education is not an accident. A partial explanation has been given in the preceding paragraph. To understand the situation better one should consider the attitude of teachers of graduate work, for their influence upon higher education is important. Why has there been a considerable number of graduate teachers in favor of the removal of the requirement of an original thesis for the master's degree? Why is it that in some subjects there have been so many degrees conferred that the doctor's thesis has not been universally treated as a golden opportunity for the fixation of an alert attitude toward creativeness? The most evident reason for the failure to emphasize the importance of experience in creativeness on the part of both undergraduates and graduates is that the specialist sees as his duty the conveying of information to the ordinary student and stimulating only the unusual case who may desire a career in the teacher's field of study. He relates creativeness only to his own field. The usefulness of the creative attitude in all types of work in the world escapes the consideration of the specialist. Consequently, it does not influence the goals he sets in undergraduate higher education.

At this moment when the United Nations are considering so earnestly ways and means of cooperating for the sake of civilization, and when we in this country are at least threatened with a postwar recurrence of depression, surely educators should give thought to the influence of the development of the attitude of creativeness in the mind of the ordinary man. Will it increase his earning power? Will it increase the number of jobs available? What influence will it have upon happiness and satisfaction in life? What impetus will it give to the possession of purposefulness in life? If one carefully considers the answers to these questions, he will conclude that higher education does have laid at its door the responsibility not only for the usual preparation of college graduates as stated in the curriculum catalogs, but also for the supplying of experience in creativeness in connection with the regular courses of study.

If higher education should take an active interest in the cultivation of creativeness through experience in practice, what is the contribution that physics can make to this end? What can the teacher plan to do if he now adds to his goals in teaching the opportunity of practice in creative thinking? The first step is a conviction that experience in creativeness is an important part of the college education of the ordinary man. Unless a teacher is duly impressed with this importance, he will not devote the time or the knowledge requisite for any plan he may devise. Not only must he be willing to spend additional time, but also he must lay aside, to some extent, his former routines. A physicist is a specialist and as such relies on the process called teaching, upon his explanations, his lectures, and his enthusiastic interest. He really takes seriously his own part on the stage. He clarifies, he emphasizes the association of ideas, he points out the reasoning and the accuracy involved in physics. He makes the laboratory a place for lively interest in the cultivation of basic principles in concrete experiments. If the teacher now adds the opportunity of creativeness to his goals, this will probably involve his "stepping out of character" as a teacher only and becoming more an observer.

An illustration will explain the careful planning and the additional labor involved if the teacher attempts to supply experience in creativeness. The prospect is attractive only if there is confidence in the importance of the result. The author has for more than 18 years taught an elementary course in acoustics to students in the junior and senior years in music (for whom it is a required course) and to graduate students in music, psychology and speech. Few of these

students have had either high school or college physics, or college mathematics. The results obtained from this practice in creativeness in the course have been helpful to the students and really inspiring to the teacher. The transformation in the students from a belief in the impossibility of "thinking scientifically" to a thrill from doing that very thing, is very marked. Of course, it is a class so selected that points of individual interest can readily be found. The conditions are favorable, at least in part, but there are also definite prejudices and indifference to overcome. Nevertheless, the effort to give practice in creativeness succeeds in all but a small fraction of the cases.

The plan used in elementary acoustics is quickly described. About the mid-point of the course each student in the class of 25 to 40 is interviewed to find his chief interests. This may require one and sometimes more than one conference, each of 15 or 20 min in length. Assume that the individual is especially curious concerning a certain musical instrument. The instructor is presumably familiar with the published scientific studies, and their availability in the library. He suggests a *limited* assignment of study for the student and encourages the preparation of a report which will be not merely a résumé of the published material but, more importantly, also the application of the principles of acoustics (studied in the course) to the selected aspect of the musical instrument.

The student at first doubts his ability. He can understand the literature if it isn't too technical, but how can he do independent thinking? If he takes courage, however, he finds to his delight that he can reason from effect to cause and that knowledge of physics can increase his interest in his instrument. The report he prepares may not be new, may not even be correct in detail, but he has had the chance of practice in creative thinking. The opportunity for his own contribution is emphasized in all consultations. The teacher does not write the report, but, through repeated brief conferences he makes certain suggestions as to scope and emphasis. Then, during the last few weeks of the course, brief five- to ten-minute abstracts are given to the class by the authors of the reports. The listeners are required to submit in writing unanswered questions suggested by these abstracts.

It is noticeable that the student's general confidence in the use of his mind increases under this treatment. He finds that an ordinary man can use his knowledge in an original way. There is a definite change in his attitude toward knowledge and the application of it. Where does the student find time to do all this? Since the student will acquire more education through this experience than would be possible in the same amount of time spent in the regular assignments in the text, the learning process is abbreviated for the independent effort.

The general spirit in the foregoing illustration can be employed in any part of elementary physics, if the instructor is sufficiently well informed. Even the lack of adequate library facilities might require more thinking on the part of the student. There is one serious obstacle to the success of any such effort in physics, and that is the instructor's opportunity to devote the time and personal interest necessary. In large classes in a university where mass instruction is practiced, the effort described may be almost prohibitive. Yet the instructor will get great satisfaction in struggling to accomplish whatever he can in the practice of creativeness.

What specific conclusions from the afore-described teaching experience seem to be applicable in any similar effort, for example, with a class in elementary college physics in a general education program? *First*, evidently a study by the instructor of the individual interest of each student must be made. This is fundamental. *Second*, this interest must be found, not by mere questions, but through conferences covering a wide range of suggested topics with which the teacher is familiar. This may take a great deal of time. *Third*, it must be recognized that there is no general method that can be planned for all. The effort cannot be made to succeed automatically. Individual instruction of a personal kind is required. *Fourth*, the experience in creativeness should not be forced on any student. While the teacher must not be easily turned aside from his effort, he ought to be ready to desist when the individual case is too discouraging. He will count on the influence of the success of other students, which may often alter resistance. *Fifth*, only the unchanging faith of the teacher, great patience and ample persistence will win. This, then, is not a picture to attract every teacher. The goal involved is difficult of attainment but is worth a great deal of sacrifice. If classes are too large or the teaching load is too heavy, the requisite conditions stated in this paragraph cannot be met. The instructor will be given sufficient relief only if he is convinced of the importance of the effort.

The advanced courses in physics are much

more capable of modification to give experience in creativeness. The registration is small, and the students are more interested. Again much time will be required by the instructor. The suggested activity of the student should not be called "research," but merely the preparation of a report. It might have reference to a new design of laboratory or lecture equipment. It might be a new method of approach in the study of a certain portion of the subject, it might be a novel application, and it might even suggest experiments for an investigator. The amount of time that is devoted to this special work in the routine program of a course is a matter of judgment. In the experience herein related, the writer has found that a time allotment of about 15 percent can produce a well-defined result.

The influence on the instructor himself in following the foregoing suggestion is not the least commendable feature of the proposed effort to give experience in creativeness. He will need constantly to study new aspects of physics and to probe more deeply into known facts. This will have its personal rewards in satisfaction. Also, he will participate in the creative work of the student and will indulge to some extent in creative work himself. Such activity will influence all of the teaching of the instructor. He will be more interesting and more inspiring. In point of fact, a teacher's earlier life as a graduate student in a physics research laboratory usually has conveyed to him too narrow a view of what is creative. The tendency is for him to think that any subsequent creative activity of less dignity and profundity is not worth while. This is highly erroneous. In the college where he is teaching it may be impossible for him to carry on the same or a similar type of research as that in which he was engaged for the doctorate. What then will he do? He will either stagnate or, before it is too late, will decide that the only creative work worth while to any individual is what he can find the opportunity to do in his own environment. If the instructor will recognize this fact and will make the most of the opportunities that are his, he will grow in stature, will inspire his students, and the education that they receive at his hands will have a better quality. Moreover, new opportunities to be creative will emerge.

If physics can make a definite contribution to general education in its interests in cultivating creativeness, what can higher education do in an organized way? Evidently it can attempt to find what courses of study are capable of making the contribution which is herein advocated, and it can encourage the instructors in these courses to undertake this type of instruction. It is to be observed that every student carries a major subject or has an area of special interest. It is here that experience in creativeness can probably be supplied most easily. It is very clear that in literature, music and the fine arts creative endeavor is quite frequently if not universally encouraged. In this respect these subjects supply a bright spot in general education; but, unfortunately for their services in leavening the whole lump, they emphasize very unusual gifts. In many other subjects there are ample opportunities for individual interpretation which involves creative thinking. Consequently, higher education has the opportunity to take definite steps in the direction of recognizing the importance of practice in creativeness as a part of general education. But higher education should not stop here. A study of the influence of education at each of its levels upon the creativeness of the student is imperative. Just what plan should be suggested for such a study could only be drawn up through the careful and cooperative discussion of a number of educationists and other scholars who would be interested in the project. When such plans are drawn, doubtless there will be found means to carry them out.

In the meantime, teachers of physics need not wait. Our subject is rich with opportunity for experience in creativeness. If we believe in the importance of the establishment of self-confidence as one of the goals in college education, we can contribute greatly to the general education of the ordinary man.

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*[J. J.] said that the absence of personal triumph in the acquisition of knowledge was really the thing to guard against in education.*—LORD RAYLEIGH, *The Life of Sir J. J. Thomson.*

# Reality and Relativity

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ONE of the most important consequences of the theory of relativity has been its interpretation of the nature of reality. Such interpretation should have far-reaching effects upon the character of what we call the laws and facts of nature. But because the question has not been explicitly debated in most expositions of relativity, the effects which have been realized have been limited, and, indeed, many of the tacit assumptions yet made with regard to reality are not in complete accord with the principle of relativity. We shall in this paper try to reach the understanding of reality which is consequent from relativity theory, and demonstrate that this understanding is prerequisite for interpreting science as an instrument which presents a true account of nature.

The question of reality is invariably broached by the student of restricted relativity. In consideration of the physical consequences of the Lorentz equations, he will ask, "Is the contraction in length observed in a moving body *real?*" A physicist so interrogated may answer, "Yes, but the contraction is evidenced only from the point of view of the stationary observer." The student becomes perplexed. "Am I to infer that what is real for the stationary observer is not real for the moving observer?" Here the physicist will smile and say: "That is correct. Any measurement or the result of any act of observation is dependent upon the point of view of the observer. And since the observation of one observer is as valid as that of any other, provided that both observers make similar and valid observations when stationary relative to each other, we must conclude that reality, as meaning valid descriptions of nature, is relative." The physicist further explains that there can be no fixed reference system to which the motion of both observers could be referred, that there exists no unique system from which to make observations that could be considered as "absolutely real." Therefore, he calls what each observer sees real, and says that both observations together constitute a *real* picture of the world.

But if the physicist is not careful, he will find himself believing in the absolute reality of the principle, "reality is relative." One suspects this from his statement concerning the reality of the world picture of both observers. He will be, perhaps, too eager to describe the over-all relativistic picture of the world (that given by the total of all observations) as real, and he may claim that relativity theory gives an absolutely real description of the universe. He may argue that the invariants of relativity theory are absolute; that, for example, the relativistic Maxwell equations are invariant under Lorentz transformation, and hence their *application* to give real results is independent of the situation of the observer (though, of course, the results themselves are not independent). But the reality of this particular description of electromagnetic phenomena is dependent upon the reality of the principle of relativity as a theory that gives valid descriptions of nature. We shall see from the understanding of reality given further on in this paper that any theory, indeed any "fact of nature," can only be *defined* as real under certain conditions. Relativity theory will prove to be no exception, and the picture of nature which it presents we can only define to be real. We must, therefore, examine the physicist's attitude toward reality thoroughly and see whether or not he is fully convinced of the principle he offered as his belief.

He knows that this principle is a consequent of the Lorentz equations. However, he has expressed his belief in the principle only as it is applied to observations where relativistic phenomena occur. Two questions arise. First, could he believe in the principle of the relativity of reality even if the Lorentz equations were demonstrated to be inapplicable to this interpretation of reality, that is, if the inference, "reality is relative," were not a valid deduction from the Lorentz equations? And second, does he realize the full import of the principle and its consequences in regard to other physical and philosophical problems? A consideration of these two questions will, I think, give the key to the relativistic interpretation of reality which is elicited by the advancement of the theory of relativity but which then becomes independent of it.

Let us consider the question of applicability. Einstein derived the Lorentz equations by making only one new assumption, the second

postulate of restricted relativity, that the velocity of light is independent of the system of reference. Obviously, the implication is that the velocity of light is the limiting velocity in nature. The fractional contraction in length and the time dilatation in a moving system from the point of view of a stationary observer are found to be $(1-\beta^2)^{\frac{1}{2}}$, where $\beta$ is the ratio $v/c$; here $v$ is the velocity of the moving system relative to the fixed observer, and $c$ is the velocity of light in vacuum. The situation is not, however, that a meter stick simply contracts in the direction of its motion, but rather that if we try to *measure* it or *observe* it in any way (now known) from the stationary system, a contraction in the direction of its motion is evidenced. It is only when we perform the *operation of measurement* that the contraction is found.

But how else would we know of the contraction, the reader may impatiently ask. We assume that we must perform a measurement to detect it. Am I implying that if we are to believe in the existence of the contraction, we must prove it in a manner other than by observation? Not at all, but we must recognize explicitly that the method of observation plays the all-important role in relativity. We must, therefore, consider the question of whether or not our method of observation has any effect upon the result obtained.

We might measure the traveling meter stick by recording the coincidences of its ends upon a stationary scale by means of light signals. When we received a flash of light from a point on our scale we would know that an end of the stick had just been there. However, since the velocity of light is finite, we could not know instantaneously of the coincidence of the ends of the stick with points on the scale, and hence we could not know of the simultaneous occurrence of the coincidences of both ends of the stick with points on the scale. Therefore, the measurements must be corrected by the contraction factor $(1-\beta^2)^{\frac{1}{2}}$ in order for us to know the length of the stick when it is stationary relative to us.

Is the contraction thus observed in the moving stick real? We have to admit that it must be as real as the noncontraction found by an observer who may be moving with the stick and thus stationary with respect to it; for there can be no one to tell us which observation may represent the true condition of the stick and which does not. We are bound then to capitulate to an understanding of reality which allows this situation—which indeed demands that world pictures are inextricably bound up with the method and point of view of observation, that they are unique for each reference system, and that the picture given by one coordinate system is as real as that given by any other. Of the reality of all of the pictures taken together, we can say nothing, since this would require an absolute observer to make any statement. Thus, our hypothetical scientist would be at best redundant and at worst metaphysical in calling the infinity of pictures taken from infinitely many coordinate systems real. It must be evident from this discussion that the concept of reality can be nothing more than a *definition;* in other words, we will make a definition that will be congruous with the situation which, as shown from the Lorentz equations, exists in our perception of the universe. Let us, however, wait a moment before formulating it.

What if we could change our method of measuring so that we could instantaneously record the coincidences of the ends of the meter stick upon our fixed scale? No contraction would be observed since $c$, the velocity with which the fact of coincidence is transmitted, would be infinite, and $(1-\beta^2)^{\frac{1}{2}}$ would therefore approach unity. But, it may be argued, the velocity of light is the limiting velocity in nature, and it is impossible to record the coincidences instantaneously. Though this has been demonstrated by several classical experiments—for example, those with the Michelson-Morley interferometer—we can admit only its probability and not its absolute certainty. But it is not necessary to debate this point. All that we have intended to show is that the contraction will depend upon this fact, indeed, upon the limited scope of our measuring technics.

I realize that by speaking of the limited scope of our measuring technics I may appear to be debating a metaphysical proposition, since the proposing of such limitations might imply that I am suggesting that there are other technics which we cannot achieve by experience and consequently do not know of. This is not what I would suggest or imply. My suggestion is simply that we derive knowledge only through experience, and that we can observe only by such methods as are elicited by our experience; therefore, since the range of our experience is finite, in this sense, I say that our knowledge is limited. That the velocity of light is the limiting velocity in nature was deduced by particular

observations or experiments. This "fact," being congruous with all experience, we defined to be real. One might add "real for us," but this phrase will be redundant rather than metaphysical provided that one does not postulate any conditions for reality which are not derived from his experience. In other words, when we speak of reality, we *ipso facto* mean, from our point of view. To the question, "is there a unique point of view that would give an absolutely real picture?" the answer must be, "no." The statement, "reality is relative," is likewise dependent upon the tenable knowledge of the world.

We can now amplify this principle and present it in the form of a definition, the character of which is dictated by our experience. It must answer the question, "What is the nature of that form of existence which we call real because of its verification by experiment?" In the case of the Lorentz contraction, all experiments to detect it would give the same result if made from the same point of view, or coordinate system. The question of the reality of the total of all pictures taken from many (or infinitely many) points of view we have shown to be irrelevant and meaningless. Thus we find that *we call real that which we observe to be consistent with our general picture of the universe. We call real our descriptions of the world, provided they are logically valid within the frame of the method of description employed.* We make this definition by our own design; for we believe it to be the most expedient statement of reality. That it is the most expedient statement should now be easily demonstrable by only the naivest reflection upon any other definition.

This interpretation of reality was known intuitively before relativity; for it was the only tenable understanding of reality that experience could elicit. Men may have given lip service to the principle of an absolute reality, but there can be little doubt that their knowledge of the world was developed with the aforementioned interpretation of reality as their intuitive guide. Relativity simply gives us a rational and articulate statement of what was heretofore obscured by the psychological handicap of our prejudiced search for certainties.

The import of this point of view is now obvious. We must realize that our concepts are nothing more than definitions which are derivative from our method of observation. Our definitions are simply operational descriptions. The picture of the world is then dependent upon both the method and the point of view of the observer and his consequent analysis of events. (Here I use the relativistic interpretation of event.) For example, we perform a set of experiments from which we may hypothetically infer the existence of "cosmic rays." Of course, "cosmic rays" are more than just the total of the performed experiments. Having once attributed certain properties to these "rays," we may, from the logic of physics, derive more information about their nature. We thus make predictions, and by the verification of these predictions we say that we increase the probability of the existence of "cosmic rays." Are they real? First, they have been observed, and we have had direct experience with them. Second, they are consistent with the picture of the world presented by science and that part of nature described by physics, and their description is logically consonant with science. Then, from our definition of reality, we call them real. We shall not add the restricting phrase, "real for us;" for such a statement would, as we have seen, be redundant and perhaps beguile us into metaphysical speculation.

We recognize that to have an absolute form of reality in any sense of the phrase, we would need to know of an absolute observer. That this is impossible has been demonstrated by the relativity theory. However, though it was relativity that abolished the principle of absolutism—the belief that there can be an absolute scale to which our knowledge may be referred for truth—this intelligence once realized becomes independent of the applicability of the theory to the question of reality. This is not the paradox it might at first appear. By the very nature of absolutism, only the thought of its impossibility is a potent enough weapon to destroy it; for absolutism is simply a doctrine of belief. We may have said that we believed in it heretofore; for we could not (or would not try to) consider the world without it. But now relativity has disciplined our minds in frankness. We have every reason to believe that the definition which we have formulated here will continue to be validly derivative from the Lorentz equations. But should some new measuring technic be found, we would not fall back to absolutism. Relativity theory has made our thoughts upon the problem of reality articulate. It has given us an explicit understanding that is, indeed, in accord with our implicit conception of reality.

# Continuity in Mathematics and Physics*

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*God made the integers, and man made all the rest.* These words are attributed to a great mathematician of the 19th century, especially prominent as a number theorist. Today, in the 20th century, some might be inclined to go further even than Kronecker, and say: the Creator made ten fingers, ten nuts, ten apples—but man made an abstraction, the number ten.

One important difference between modern science and the science of the classical age lies in the rise of a school of thought that has been called formalism in contrast with the intuitionism of the past. The intuitionists believe in the *a priori* existence of certain concepts and regard definitions as attempts to reproduce as accurately as possible the *a priori* concepts. According as an attempt succeeds or fails, the definition is right or wrong, and its value is judged by its success in this respect.

The formalists, on the other hand, deny the *a priori* existence of most concepts, and hold that the concepts are created by their definition. From this point of view, a definition cannot be wrong, and its value is not to be judged by comparison of the concept that it creates with something supposedly already existing, but solely by its usefulness.

Three-quarters of a century ago a great authority on heat wrote that it has not been proved that the gas thermometer *really* measures that which we think of as temperature. This is intuitionism in its full naiveté. However, about 1900, Ernst Mach,[1] in a critique of the temperature concept, took issue with this statement and urged a formalist point of view.

Further examples are not difficult to find. Surely the doctrine of natural dimensions as against that of definitional dimensions illustrates the point nicely. Or, if for an instant we go outside of our own science, is not the Gestalt theory of a few psychologists an attempt to revive intuitionism as opposed to the formal behaviorist school? I judge from a remark of Courant that in mathematics also there are a few who try to return to the earlier views.

That complete formalism is an impossibility, that, in order to define—to reduce the item to be defined to something already known—it is necessary to begin with something assumed as known, which is intuition, is strikingly illustrated by an anecdote of Boltzmann's. In his younger years he once asked his brother for the loan of an English dictionary, because he wanted to read an English philosopher in the original. His brother remarked: "You need no dictionary. If this writer is not a true philosopher, you don't want to read him, and if he is, then, as you always say, he defines every term before he uses it." The present view in science and in mathematics is a mixed one—a little intuition, as little as possible, and much formalism.

One advantage of the formalist position is, I believe, the fact that it makes men more tolerant of views other than their own. So long as men believe, as the intuitionists do, in absolute truths, they will fight for them. A violent controversy, such as that between the adherents of Leibniz, who held that the strength—*vis*—of a body should be measured by $mv^2$, and those of Descartes, who thought it should be $mv$, is unthinkable in an age in which men believe in definitional concepts.

The concept of a continuum has been under debate for several thousand years. Possibly, but I am not sure of this, the paradoxes of continuity—you will recall the case of Achilles pursuing the turtle without catching it—which the ancient mathematicians could not properly resolve, because they lacked the most important concept, namely, that of the limit, led some thinkers to abandon the continuum despite, as a modern mathematician puts it, its psychological reality in the human mind. At any rate, we find already among the ancient philosophers some who advocated an atomic world picture—atomic, of course, not in the specific sense of modern physical science but at least in the sense that they preferred to think in terms of discrete entities instead of objects divisible without limit.

Other philosophers regarded an atomic world as inconceivable and continuity as a necessity of thought. Recently I read with surprise a passage in the works of Schopenhauer, an early 19th

* From an address to the New England Section of the American Physical Society.

[1] E. Mach, *Die Principien der Wärmelehre* (Barth, 1896), p. 50.

century philosopher, who enjoyed a great vogue for a time, although we must admit that his views on mathematics and science are not authoritative, since his training was entirely philological, and his scientific views highly speculative. He held that the conservation of matter was obviously a necessity of thought, a philosophic truth, and that all the work chemists had done to put this on a firm empirical basis was entirely uncalled for, superfluous, unscientific. While he was thus prepared to accept offhand one of the fundamental doctrines of chemistry, at the same time he deplored the rise of the atomic theory, expressing the view that it must inevitably lead scientific thought and method badly astray, since matter was obviously continuous, divisible without limit, and could not possibly consist of discrete entities.

As to what is a necessity of thought, as to what is conceivable and what not, the philosophers of the past have often exhibited what must be described as mental arrogance. Certainly a considerable amount of confidence in one's own mental powers is needed to assert that only what our mind can see can be seen by others, and that we can see everything as well as others.

A modest 20th century scientist will therefore not ask: Is continuity a philosophic truth, a necessity of thought? But the question may be raised: Is the continuity concept necessarily intuitive, one of the primaries—which, it is claimed, not even David Hilbert, the most uncompromising formalist, could do without—or is it a definitional concept? To that question the general reply today is, the latter.

It does not seem necessary to repeat the $\epsilon$ and the $\epsilon$ and $\delta$ definitions that we struggled with in our student days. Many will recall Osgood of Harvard, and others, perhaps, Pierpont of Yale, and their insistence that the choice of $\epsilon$ be made first, making it as small as one pleases. But, once the choice has been made, it is final; now $\epsilon$ must remain fixed, and then $\delta$—Osgood always called it $h$—is to be chosen, and so on.

How we wondered; and perhaps we mildly questioned the necessity of all this, for did we not know already what a continuous function is? All young mathematicians are intuitionists. But when we came to problems such as: is a function that has a derivative necessarily continuous, to which the answer is yes; does every continuous function have a derivative, to which the answer is no, which, so long as one relies on intuition, seems to make no sense; and when we encountered the queer functions such as Weierstrass', $y=x\sin(1/x)$, which, if $y(0)=0$ by definition, is continuous everywhere, but has no derivative at $x=0$, or the functions of Weierstrass and Dini, which though continuous everywhere have no derivative anywhere; then we gradually began to realize the value of the formal definition, and ultimately it came to stand in our minds for the concept itself.

It might seem that the ideas of continuum and of atomicity are irreconcilable, that there is no compromise, no connecting link. That, however, is not the case. To show this connection, an illustrative example may serve better than general considerations. The example chosen is a simple one, the calculation of the rotational inertia $I$ of a linear distribution of mass about a transverse axis through one end. Of course a linear distribution of mass is an abstraction, a fiction that is no more real than is the Euclidean line that replaces the stroke of a pencil or a crayon. But it will allow us to concentrate on the question without complicating the argument with too much detail.

Let the total mass be $M$ and the length be $l$, and let the linear density $\sigma$ be constant in value. Then, if $\mathrm{d}x$ is a short element of length, and $x$ its distance from one end, we have by the continuity method,

$$I=\int_0^l \sigma\,\mathrm{d}x\,x^2=\tfrac{1}{3}Ml^2. \tag{1}$$

Before discussing this result, let us establish the expression for a discontinuous distribution. Let us quantize or atomize as simply and radically as possible by assuming that the length $l$ consists of $n$ length atoms or quanta, each of length $\lambda$, and each having its mass $\mu$ concentrated at a point, say for convenience, though this is not necessary, the end of $\lambda$ farthest from the axis of $I$. Then we have

$$\begin{aligned}I&=\mu\lambda^2+\mu(2\lambda)^2+\mu(3\lambda)^2+\cdots\\&=\mu\lambda^2(1+2^2+3^2+\cdots).\end{aligned}$$

Now the sum of the squares of the first $n$ integers is given by

$$(n^3/3)+(n^2/2)+(n/6).$$

Thus we find, upon replacing $n\mu$ by $M$ and $n\lambda$ by $l$,

$$I=(Ml^2/3)+(\mu l^2/2)+(M\lambda^2/6). \tag{2}$$

Clearly, Eq. (2) is in substantial agreement with Eq. (1) only when $\mu$ is small compared with $M$, and $\lambda$ small compared with $l$, a point to which we return again.

That Eq. (1) involves the assumption of a continuous distribution is evident for two reasons: first, the very process of integration is closely associated with the idea of a continuous function; and second, for a discontinuous distribution the density $\sigma$ does not properly exist.

Formerly, and in some textbooks today, Eq. (1) is described as giving the rigorously correct answer and Eq. (2) as an approximation to be used by those who know no calculus. If, however, the continuity assumption is not made, then Eq. (2) is the correct answer, and Eq. (1) is an approximation to be used by those who do not know enough algebra to evaluate the sum in Eq. (2) and have to resort to calculus, because it is easier. The number of those who prefer the relatively simple technic of integration to that of summation is not small; my own students, at least, always smile when I refer to various summation formulas as being well known, but they can integrate.

How the summation may often be replaced by an integration is illustrated by Eq. (3). The process, which Darrow has called "smoothing over," is frequently legitimate. Choose an element of length $\Delta x$, large enough to contain many length atoms each of length $\lambda$; then the number of such atoms in $\Delta x$ will be $\Delta x/\lambda$, and the mass in $\Delta x$ will be $\mu\Delta x/\lambda$. Upon multiplying each mass by the appropriate $x^2$ and adding, we obtain

$$I=\sum \mu(\Delta x/\lambda)x^2. \tag{3}$$

Since the total mass $M$ is equal to $l\mu/\lambda$, we may write Eq. (3) in the form

$$I=(M/l)\sum x^2\Delta x.$$

If now we were to replace the sum by the definite integral $\int_0^l x^2 dx$, we should obtain for $I$ the same answer as in Eq. (1), namely, $\frac{1}{3}Ml^2$. Is this replacement permissible? Although, strictly speaking, a sum is not equal to its limiting value, nevertheless the relative difference between the sum and the integral will be very small if two conditions are met; namely, $\Delta x$ should be sufficiently small compared with $l$, and sufficiently large compared with $\lambda$. If, for example, $l$ were $10^6\lambda$, $\Delta x$ might be suitably taken as $10^{-3}l$.

If, on the other hand, the atom $\lambda$ is too large, or, what comes to the same thing, the number of the atoms is too small, the smoothing-over process fails, because no element $\Delta x$ of a suitable size can be found. Discontinuous systems that cannot be treated by the smoothing-over process with the replacement of a summation by an integration may be called degenerate.[2]

In statistical problems the atom or quantum becomes a cell of phase space, usually the Planck constant $h$, or some power thereof, and the quantity corresponding to our previous length element $\Delta x$ is what Darrow[3] calls a region or shell. The condition for nondegeneracy, that is, that the region must contain many cells, which is simply $\Delta x \gg \lambda$ in the elementary problem, now becomes[4]

$$n\frac{h^3}{V}\frac{1}{(2\pi mkT)^{3/2}} \ll 1 \tag{4}$$

for ordinary gases treated by Einstein statistics. In this expression, $n$ is the number of the molecules, $h$ the Planck constant, $V$ the volume of the gas, $m$ the mass of a molecule, $k$ the Boltzmann constant, and $T$ the thermodynamic temperature. The classical statistics, if the Tetrode term is included, leads to the same criterion,[5] except that the left-hand member contains the numerical factor $1/e=0.368$; and the Fermi statistics for an electron gas, which will be considered later, contains an extra factor $\frac{1}{2}$.[6] Since $V(2\pi mkT)^{3/2}$ is of the same dimension as $h^3$, namely, (distance$\times$momentum)$^3$, and since $n$ is a large number, one vaguely recognizes the condition that the quantum cell $h^3$ must be very small.

To put this criterion into a simple form, one may proceed as follows. Introducing a length $l$, defined by $l^3=V/n$ so that $l$ is of the order of magnitude of the distance of the particles—the edge of a cube if the particles are arranged in a cubical pattern—and taking the cube root, we obtain in place of Eq. (4),

$$\frac{h}{(2\pi mkT)^{1/2}l} \ll 1. \tag{5}$$

If next, with Bohr, we put $h/2\pi$ equal to the angular momentum of an electron in the innermost orbit of a hydrogen atom, write for the mass of the electron $\mu$, for its linear velocity in the first orbit $v_1$, and for the radius of this orbit $r$, we obtain

$$h/2\pi=\mu v_1 r. \tag{6}$$

[2] M. Planck, *Wärmestrahlung* (ed. 4, 1921), §134.
[3] K. Darrow, "Statistical theories," Rev. Mod. Physics **1**, 90 (1929); Bell Sys. Tech. J. **22**, 108, 362 (1943).
[4] Mayer and Mayer, *Statistical mechanics* (Wiley, 1940), chap. 16.
[5] M. Planck, reference 2, §182.
[6] A. Sommerfeld, "Zur Elektronentheorie, . . . ," Zeits. f. Physik **47**, 1 (1928).

Since for a gas molecule of mass $m$ and velocity $v$ the energy is

$$\tfrac{1}{2}mv^2=\tfrac{3}{2}kT,$$

the momentum $mv$ becomes

$$mv=(3k)^{\frac{1}{2}}m^{\frac{1}{2}}T^{\frac{1}{2}}. \qquad (7)$$

Introducing the results of Eqs. (6) and (7) into Eq. (5), we find

$$(6\pi)^{\frac{1}{2}}\mu v_1 r/mvl \ll 1. \qquad (8)$$

Thus the criterion for nondegeneracy has been reduced to the consideration of the ratio of masses, velocities and distances, all of which have simple meanings.

As an illustrative example, let us evaluate expression (8) for oxygen under standard conditions of pressure and temperature. In that case $m$ is $32\times1853\mu$, $v_1$ is approximately 1/137 of the velocity of light, or $2.2\times10^8$ cm/sec, $v$ is $46\times10^3$ cm/sec, $r$ is about $0.5\times10^{-8}$ cm, and $l$—obtained from the volume occupied by one mole, $22.4\times10^3$ cm$^3$, and the number of molecules in a mole, $6.1\times10^{23}$—is about $33\times10^{-8}$ cm. Thus the left-hand member of expression (8) is about 0.0055. This is small enough to show that standard oxygen certainly does not degenerate.

Even at very low temperatures oxygen would not degenerate. Suppose it were cooled from 273° to 1°K. This would make the $v$ in the denominator 17 times smaller, the velocity changing with the square root of the temperature. But the entire fraction would not become 17 times larger, for in order to remain a gas at 1°K, which is far below the critical temperature and the normal boiling point, the oxygen would have to be at low pressure and large volume, which would increase the value of $l$ in the denominator.

The latter effect would be much less marked in the case of helium at 1°K, because for helium this temperature is not nearly as much below the critical temperature and the normal boiling point as it is for oxygen. Moreover, the momentum $mv$ of helium under standard conditions is only about one-third that of oxygen. Nevertheless, even helium would still satisfy the criterion for nondegeneracy at 1°K.

At a time when the existence and significance of $h$ was not known to science, statistical methods were developed in connection with the kinetic theory of gases on the assumption that the cells of the six-dimensional momentum-coordinate space could be made any size, without a lower limit. Since, as is known now, ordinary gases do not degenerate, this procedure produced correct results. When, however, the same procedure was applied to conduction electrons in metals, and they were treated as constituting an electron gas in a space filled with obstacles, the procedure failed. This is because the electron gas does degenerate.

In applying the criterion (8) to an electron gas, the constant factor $(6\pi)^{\frac{1}{2}}$ must be divided by $2^{\frac{1}{2}}$, because, as previously stated, Eq. (4) contains an extra factor $\frac{1}{2}$ on the left-hand side. For the rest, the ratio $\mu/m$ becomes unity, and the ratio $r/l$ for a good conductor like silver, calculated from the volume of a mole on the assumption that the number of the conduction electrons is substantially equal to that of the atoms, becomes $\frac{1}{6}$. If the value of $v$ for an electron gas at about 300°K is calculated on the same principle as that for an oxygen molecule, the ratio $v_1/v$ becomes about 20. It is true that this calculation cannot be expected to give more than a very crude approximation, since in a degenerate system the average energy of a particle is not equal to $3kT/2$. However, since the rest of the left-hand member of expression (8) is near unity, nondegeneracy would require that $v$ be considerably *larger* than $v_1$, perhaps one-tenth of the velocity of light. Since we do not believe this, we are forced to conclude that an electron gas will always be degenerate, except at temperatures so high as to be entirely outside of the range of experiment.

Many times in the past, speculative thinkers have obtained answers to their own complete satisfaction to certain problems. Later, empirical scientists, in the endeavor to construct a system into which the factual empirical knowledge can be fitted, have been compelled to discard the speculative systems. Galilei's contemporaries were quite convinced that, of course, heavy bodies fall faster than light ones; 19th century philosophers wrote papers to show that all science in the last analysis must be based on mechanical models; and even as great an intellect as that of Auguste Comte predicted the future course of science incorrectly.

If we admit that the Planck constant $h$ is indispensable for a complete classification and systemization of our present-day knowledge, we are forced to the conclusion that we can no longer accept the idea of a perfect continuum. The existence of $h$ as a natural limit to the size of an element of the product of momentum and distance, which comes closer to quantization of time and space itself than atomic theories of matter and electricity, the necessity of considering, in certain cases at least, degenerate systems, removes from speculative philosophy the question of whether science is concerned with continuity or with atomicity, decides against continuity and makes Schopenhauer's assertion that continuity is a necessity of thought another philosophic truth that is experimentally false.

# The Physical Sciences in General Education

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THE role of the physical sciences in a general, or liberal education must depend on what kind of education a general education is supposed to be. If it is assumed that a general education is the education which everybody should have, then the nature of the education and the role of the sciences in it is determined, at least in part, by this premise.

Only a few institutions of higher learning, however, are committed to the philosophy of a required and common education preliminary to specialized work. In many, students may choose between scientific and nonscientific studies almost in advance of any study. Nevertheless, there seems to be considerable belief among teachers of physics that there are values in the study of physics which nonscientists, and therefore everybody, should have. This belief finds expression, frequently, in the organization of special physics courses for nonscience majors. If teachers of science will grant the validity of a corresponding belief on the part of teachers of the humanities and the social sciences that the study of these disciplines has values which scientists should have, the foregoing definition of general education will appear less arbitrary than it might otherwise seem to be.

A general education may well be the final, formal education of most of those who get it, and must therefore provide that degree of knowledge and understanding of natural science which the nonspecialist member of the community requires. He does not, like the professional scientist, produce scientific knowledge, but, on the contrary, is the recipient of it. He must act or make decisions on the basis of it. This demands, first, the habit of seeking such knowledge and skill in finding it; and second, the habits and skill of appraisal and criticism. Action, particularly, requires analysis of the validity of different kinds of knowledge and of their ranges of application.

The introductions to textbooks frequently remind us that men should understand the world they live in, that there is a certain amount of information about the world which everybody should have—acquaintance with the major theories, mastery of the fundamental ideas, and so on. This is certainly true, but it is not enough. The citizen is concerned not only with scientific conclusions, but with their interpretation. It is important for him to know the kinds of question that natural science can answer and the limitations of the answers. The claims made for and against science, especially in times of crisis such as war or widespread economic dislocation, cannot be assessed intelligently in terms of Newton's laws of motion. It is certainly true that the student should get as complete a world-view as it is possible to communicate in an elementary course; but this does not imply that it be an oversimplified and predigested one, nor that it be obtained at the expense of understanding of the methods which have produced that view. The prevalent faith in scientific method based on the enormous success of its technologic applications might better be founded on insight into its nature. All of these statements are very general, but they have a real consequence with respect to the manner in which the sciences should be taught.

If method is truly important, then it is not sufficient to point out in an introductory chapter or lecture that the elements of scientific method are experiment, hypothesis, prediction, and so on. The best way to comprehend a method is to practise it. Now this is what candidates for higher degrees are supposed to do, and it is said, with much truth I think, that no one really understands a subject matter who has not undergone the baptism of solving a research problem. The complicated experimental and mathematical technics, the stubbornness of apparatus and equations, the variety of extraneous effects, the approximateness of data, the multiplicity of hypotheses, or the lack of them—all are part and parcel of the scientific process. Surely the student cannot be exposed to all these. But if phenomena are disordered and complicated, and the methods of imposing or finding order are various and difficult, in what sense does the student understand science if he is not aware of this? The demonstration experiment which generally works, the laboratory experiment of two hours duration which validates the theory, the problem which neglects friction, are well-tested teaching devices that exhibit the skill of the magician without revealing his secrets.

What should be the content and teaching ma-

terials of a course that attempts to reveal scientific questions and methods as they really are? The student cannot solve new problems; the problems presented will be new to him. It is not difficult to reach agreement on what these should be. The age-old questions as to the nature of matter; the causes of its variety and change; the nature of fire, heat, sound and light; the relationships of the heavenly bodies, to name some in the physical realm, will always be fundamental questions about our ordinary experience.

The questions themselves must be clearly revealed. It often seems that students find themselves deep in the answer to a question without realizing what the question is, or that anybody has asked it. The phenomena must be exhibited in demonstrations and in the laboratory, but not in such a way as to betray the theoretical preconceptions of the instructor. He knows what variables have proved significant. The student does not know, nor did the original investigator; the very description of the phenomenon depends on what aspects of it are selected. An extreme case in point is a living organism which can be viewed in many ways.

There remain the answers to the fundamental questions; namely, the major scientific theories. The central suggestion of this paper is that the primary textual materials should be the original works of the great scientific thinkers and investigators, the men who asked and answered the questions. The use, in this connection, of textbooks, exercises, lectures, discussions and motion pictures, is necessary and important, but these have the same relationship to the theories as the description of a volcano has to the volcano. The discovery, at one and the same time, of what questions the scientist asks, what phenomena he considers, what hypotheses he proposes, what evidence he collects, the predictions he makes, the logical structure of his thought—in brief, his method—this discovery is to be made by reading his works. Furthermore, from the particular mode of presentation adopted by a writer who is expressing his own ideas, it is sometimes possible to detect the implicit elements in his thought, the assumptions and preconceptions which determine or influence his selection of phenomena or his methods of classification, formalization and experiment, which in turn affect the form of his conclusions.

Of course, a number of problems arise. Many of the theories taught in elementary physics courses were formulated in the 17th and 18th centuries. Many of the ideas and experiments, to say nothing of the terminology, are obscure. The instructor must sometimes depend on historical scholarship in his own consideration of this material. Nevertheless, my experience has been that this is a minor difficulty. Some editing in the form of omissions, addition of a glossary or a few explanatory sentences often suffices. It is important to avoid the intellectual paralysis at the sight of strange terms which exposure to textbooks alone is liable to produce. After all, it should be remembered that the habits and skills involved in first acquiring and then analyzing the meaning of an author, in first accepting the author's vocabulary and then translating it, are fundamental educational objectives, more general than those involved in scientific education.

With respect to the selection of the source papers, the theories that are in the mainstream of evolution of those currently accepted readily come to mind. It is very useful, however, to study some that are not. The theory of phlogiston is an example. The nature of evidence, the criterions of truth in a theory, become more patent in such a study than in any other. The defense of a false theory by the instructor destroys his role in the eyes of the student as eternal protagonist of what is correct. A spirit of faith in the classroom has to be supplanted by one of criticism. It is to be emphasized that the historical interest of the older works is in no sense the reason for their use. They are chosen because of their scientific significance. The historical aspect is even irrelevant to one of the tasks at hand, which is the analysis of a method of obtaining an answer to a question within a certain framework. Furthermore, papers need not, by any means, be read in chronological order. Some aspects of a work can best be understood by prefacing it with a later account, other aspects in terms of an earlier one.

The grouping of the papers need not be in accordance with the current classification of subject matters. Many of the great scientific questions do not lend themselves to ready classification as physical, or chemical, or biological. The query, "What is the nature of the air we breathe?," was basic in atomic and molecular theories, in studies of pressure and in the discovery of new gases and gas reactions, as well as in theories of respiration. In a course oriented around the fundamental questions, partition of the material into the conventional subject matters introduces a specious disunity which then has to be bridged.

In conclusion, it is maintained that experiments and writings, carefully selected, arranged and adapted if necessary, are the means by which to achieve the objectives of scientific education in general education. These remarks stem from our trial of these teaching methods in the two-year physical science course in the College of the University of Chicago.

## Selling Physics to the Liberal Arts Faculty

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THIS discussion has to do with two separate subjects. One is *selling*, the other is *physics*. No salesmanship is in the long run successful unless the product is worth selling. We have therefore on the one hand to consider the prejudice existing in some quarters against recognizing the cultural value of the study of physics, and the possible technics for overcoming this prejudice; and on the other hand we must do some soul-searching to decide whether we know what the cultural values really are. We need to ask ourselves how well our teaching is succeeding in weaving our subject into the general fabric of a liberal education, and to what extent we as individuals deserve to be regarded as examples of liberally educated persons.

We all believe that a general course in physics has a large cultural value by its own right. If student advisers are not sufficiently convinced of this, we should use our best eloquence to persuade them. Nevertheless, beyond the cultural value that is intrinsic in the science there lies the importance of our recognizing, and emphasizing in our teaching, certain broader relationships.

These considerations are applicable more or less to all the natural sciences. If the situation is a little more serious with regard to physics, it is probably because physics, in its elements at any rate, has the least obvious relation to animate life and is the science most closely associated in the public mind with engineering applications.

That some acquaintance with science and with the scientific way of attacking problems is an indispensable feature in modern education is generally recognized by educators everywhere. They know, though many regret it, that our present civilization is largely dominated by science and technology. Teachers of the humanities deplore the fact that the "scientific method" has so permeated even their own fields as to diminish the appreciation of esthetic and emotional elements. They acknowledge the value of scientific research, but they think of it as something that concerns a few specialists who, like athletes, are good in their way, but whose way has little to do with a liberal education. As a palliative there is an increasing demand that the teaching of science be approached historically and philosophically as well as from the more purely technical direction.

If we would have our colleagues in the humanities more sympathetic with us, we must understand and sympathize with their point of view. Moreover, we must not only talk about the cultural values of the sciences, but so teach that our students will gain a clearer vision of the beauty and harmony in science and of its place in the scheme of life.

Specifically, we cannot expect student advisers to see the desirability of urging their advisees to elect courses in physics if we confine ourselves in the classroom to teaching the bare subject matter of courses, however well we may do it, and leave it to our students to absorb as best they may the cultural values of the subject and its relation to wider realms of thought.

Our critics have here touched a real deficiency in our methods of teaching physics, a deficiency that is being recognized more and more by physicists themselves. For example, it has recently been asserted that "we have all become primarily subject-matter specialists, and only secondarily educators;"[1] and that "a teacher should be capable of thinking about the meanings and the application of his science, not merely of teaching courses."[2] Perhaps we have felt so sure of the cultural values of our subject that it has not seemed necessary to render these values explicit. Perhaps we have felt that "art is long and time is fleeting," in the sense that there is a lifetime for becoming cultured, but only 50 brief minutes available for teaching physics in each period. How often have we entered the classroom with a glow of satisfaction over the apt analogies, the historical allusions, the gems of philosophy

[1] L. W. Taylor, Rev. Sci. Inst. **15**, 288 (1944).
[2] President Isaiah Bowman, Annual Report for 1943, Johns Hopkins University.

with which our minds were charged almost to the sparking point, only to have the minutes fleet past and the inexorable bell ring before a single pearl from our precious cargo had been discharged!

Whose fault was it? That of the college, for allowing only three periods a week for a two-semester course in general physics? Or did the trouble lie in the use of a textbook so full of material that a comprehensive perspective was impossible? Or in the use of examinations of the nation-wide type, with the accompanying urge to bend every effort toward preparing the students for those examinations? Shall we adopt Dushman's slogan[3] and teach "less and better?" These are pressing questions, and they concern both majors in physics and physics students who major in other fields.

Although these questions have a bearing on the attitude of faculties toward the place of physics in a liberal education, the answers cannot be attempted here. It might be a step in the right direction if in every college courses were offered, and perhaps required, in the history of scientific thought and in the effects of science and invention on civilization. Still, even if such a course had to do chiefly with the science of physics, it would not absolve any teacher of the subject from the duty of integrating every course that he taught into the great body of scientific thought and of human life. On the contrary, the existence of such a course would make it all the more incumbent on us to show how each individual branch of physics fits into the general scheme.

The immediate problem, then, is to suggest how the physics teacher can, while teaching his subjects as thoroughly and conscientiously as possible, at the same time make them living contributions to education in a more liberal sense. The first thing to recognize is that our success in awakening in students an appreciation of the broader aspects of physics depends on how much evidence of broad culture they can detect in us. You can't fool all the underclassmen all the time. Not that we must be masters of large areas in the fine arts, history, economics and belles-lettres. But if we do not know something about these fields, and, what is more important, have no genuine interest in any of them, so much the worse for us and for our teaching and for our success in attracting more young men and women to the study of physics.

In class we can manage, without serious loss of time, to point out repeatedly how our science demands clear thinking, intellectual integrity, the ability to analyze a problem and to synthesize a defensible conclusion. By calling for oral and written reports we can give training in the art of communicating clearly what has been learned. Here and there we can in a few words direct attention to the historical development of the topic under discussion, or suggest relationships between it and other sciences, or the arts, or philosophy, or the social or economic order. Such brief suggestions may arouse more thinking than would a lengthy exposition.

So far as there is opportunity—and usually there *is* opportunity if we are alert—we ought to follow up these matters in conversations with students out of class. A few helpful and encouraging words can act as a powerful catalyst. Friendly personal contacts will be especially important with the men now in service when they return to the campus. In particular, we ought to stand squarely with the advisers of undergraduates in cautioning against overspecialization and in advocating a balanced group of subjects in all the fundamental liberal arts fields.

Perhaps it is not out of place to suggest that if the teacher of science has sufficient command of the scientific method to teach it as a discipline that can be applied to situations in everyday life, he is inconsistent if he fails to discharge his own civic responsibilities. By willingness to participate in civic affairs, or to give popular scientific talks to local organizations, he will be accepting a responsibility as a citizen that is no less his than that of the business man or lawyer. Too often the excuse that one cannot do one's full duty in science without working nights and holidays, and that therefore one should not be expected to be a good family man or an active citizen, is a means for escaping into a tower of ivory or of other insulating material where he can do as he pleases.

All this has been said in the effort to suggest some of the means whereby our teaching of physics in a liberal arts college can be made effective in the best sense. So far as selling physics to the faculty in general and to student advisers in particular is concerned, every instructor in the department is a salesman, and every student is a sample of the product. By capturing and translating into action the vision of our responsibility, we shall not find it too difficult to convince our colleagues that if they will advise students to elect physics, we can deliver the goods.

[3] S. Dushman, Am. J. Physics **12**, 222 (1944).

# A Cultural Course in College Physics for Nontechnical Students

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A CULTURAL course in college physics for nontechnical students was begun some 20 years ago as an experiment in the general education of students of the College of Arts and Sciences. The course is not a substitute for the standard five-credit course required of engineering, premedical and science-major students. For such technical students there is no evidence that any basic change in method of instruction is either needed or desirable.

The course described herein is designed for a large and increasing number of students of liberal arts, commerce, law and education, who seek a general education but who do not have, and do not wish to acquire, the technical skills of any laboratory science. They never expect to use or need such skills, and they recognize that individually they have no special aptitudes for acquiring such technics. These students usually have a very limited training in mathematics, and even less knowledge of the scientific method. Left to their own devices, they would carefully avoid any and every course in science. Many have done just that in high school, and hence enter college with little or no science or mathematics beyond elementary algebra. If compelled by college regulation or group requirements to select a science course, they invariably choose the one that is easiest, shortest, has the least amount of laboratory, and in which a failure will be the least costly in time and inconvenience. In such cases there is rarely any real attempt to profit by the course they are required to take since they feel a resentment because science was forced upon them. When forced into a regular old-line five-credit course in college physics, their lack of training in mathematics at once becomes painfully apparent. Recitations become a battle of wits between the instructor and the student—the instructor, probing by questions the sensitive mind of the student, who often has an inferiority complex relative to science, for information that is not there, and the student using all his skill and mental ability to hide his ignorance. The effect of such recitation is not to instruct the student, but it does develop in him a dislike for the teacher and distaste for the subject.

Laboratory instruction for such a student is a waste of time. The net result for the student is a confirmed dislike of physics, a belief that the "subject is difficult," "not worthwhile," and "to be avoided at all costs." This last idea, by way of advice, he passes on widely to his fellow students. Such adverse advertisement of science often begins in high school, so that the student upon coming to college, while he may not know what courses he wishes to take, is sure that he does not want any science, particularly physics.

Some 21 years ago every Arts and Science student at the University of Kentucky who took physics was *required* to do so. Not one student in the whole University *elected* to take physics on his own initiative. It was to remedy this condition that a special course was begun as an experiment in education for cultural value rather than technical content.

The course requires three periods per week for three quarters, and consists of about 100 carefully prepared demonstration lectures, covering the whole field of physics. No individual laboratory work is done. The whole course is centered about the scientific method as a mode of thought in acquiring new information and in integrating knowledge of the material world.

No particular textbook is assigned, but on the first day of each quarter the student is given a "Schedule of lectures" showing the full scope of the quarter's work. Each day in the lecture room the student is provided with a "lesson sheet for the day." This lesson sheet presents:

(1) A brief synopsis of the lecture, making further note-taking unnecessary unless the student particularly desires to do so, in which case provision is made on the lesson leaflet for it.

(2) Some four to six references to books of elementary grade; these books in sufficient numbers are available in the reading room of the University Library. Many references are to books that would never be used as college textbooks and thus would not ordinarily be seen by the average college student.

(3) A check list of questions covering the lesson. The intent is to encourage reading as a means of seeking the answer to questions of interest.

(4) Drawings and illustrations of apparatus used. These are designed as an aid in recalling demonstrations in the lecture.

The student hears the lecture and is warned that he is expected to read the assigned references

for that day *before* the next lecture. He is encouraged to seek the correct answer to the suggested questions and to learn how to express satisfactorily, in scientific terms, the major ideas included in each lesson.

Some 33 lectures per quarter are divided into four groups, and upon completion of each group a written test is given. Each student is required to take four written tests per quarter. Before each written test, a question period is held, outside of class schedule, at which time the instructor meets all students who wish to come, and discusses any questions they wish to bring up. Such sessions often have 75 percent of the total class present, and have been known to last for three hours.

**Advantages Claimed for Such a Course**

(1) Such a course emphasizes the scientific method by constantly using it in presenting effective demonstrations, thereby suggesting its cultural value as a proper approach to the solution of any problem.

(2) It develops in the student an increased intellectual curiosity relative to the physical world. If culture may be defined as the efficient relation of man to his environment, this development of intellectual curiosity has great cultural value.

(3) It encourages—in fact, it demands—wide reading, far beyond the usual range of regular science students. This reading, which at first, perhaps, is done by the student as a necessity in order to pass the written tests, finally confirms in him a habit of wide reading and encourages a beginner to seek the aid of the library and to understand its methods.

(4) It permits much more extensive demonstrations than is possible in the usual type of physics class. Such experimentation, fully explained, often quantitatively demonstrated, with apparatus that always "works," is a very adequate substitute for any laboratory experiment that would be within the ability of this type of student to do for himself.

(5) It brings fundamental science within the range of many who otherwise would not take any natural science.

(6) It makes friends for science among those who, if compelled to take the usual course, would do so against their will and might thus become embittered because of the imposition of requirements.

(7) It results in a wide coverage for physics of the student body in the institution. For the past 14 years there were registered in this course from 9 to 13 percent of the entire enrolment of the University of Kentucky, including all five colleges and the graduate school. This, coupled with the some 12.5 percent of the entire enrolment of the University who take the laboratory courses, means that about 25 percent of the total University student body is taking physics at any one time. Hence, on the average, all students who remain in the University four years have had at least the equivalent of one course in physics.

(8) Because the course can handle large numbers of students, it is an *inexpensive* method of instruction from an administrative point of view. However, because of the wide coverage of the student body, larger expenditures would be justified than are at present made.

**Things This Course Does Not Do**

Such a course is not a substitute for the regular, general college course in physics for those who need or desire such technical instruction. Students at the University now often elect the regular course. The cultural course has *not diminished* the enrolment in the laboratory courses during the 14 years immediately preceding the present national emergency. While the cultural course enrolled from 9 to 13 percent of the total University student body, the other regular courses of the department of physics have increased their enrolment from about 10 percent to an average of more than 12.5 percent.

This course does not produce any engineers, physics or chemistry majors, or premedical students, but it does contribute to the cultural background of many who go into law or journalism, or who may major in history, political science, economics, and so forth. From this group later come members of the legislature, governors, editors, lawyers, members of boards of trustees, and other public officials. These men make the laws, administer the law, guide public opinion, control educational institutions, and determine public policy. We submit that it is highly important for them, for the State, and for the science of physics, that they have *some* knowledge of the scientific method *as exemplified by physics*, coupled with a warmhearted appreciation of what physics has done and can do for the betterment of man's cultural inheritance.

Such a lecture-demonstration course requires the construction of a wide range of effective demonstration apparatus. Apparatus to be effec-

tive must be sufficiently large *to be seen* by every student. Equally important, it must be as precisely made as if for research work. This requires the service and experience of a skilled instrument maker and a well-equipped shop. The course cannot be once organized and then left to run of itself, or be turned over to a relatively inexperienced or unenthusiastic teacher. It is necessary that both the course and the equipment be under constant scrutiny for improvement, the less efficient portions being removed and the improved and more effective methods and equipment substituted. It must be kept *up to date.*

Finally, it may be suggested that in any institution where the department of physics is ministering to considerably less than 25 percent of the total enrolment of the institution, it would seem probable that an opportunity is being neglected to present physics for its cultural value as well as for its technical content.

# Instructor Opinion on Characteristics of a Good General Physics Textbook

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DESIRABLE characteristics of a college physics textbook are often the subject of much thought and discussion by physics instructors, but any conclusions reached are generally not available to other physicists. It occurred to the writer that a poll of the opinions of instructors might be helpful and interesting to physics teachers and to authors of physics textbooks.

A questionary, which is reproduced below, on characteristics of a good physics textbook was therefore designed and sent to college physics departments. The items therein represent questions of the author and questions prepared in the light of comments and criticisms which he has heard other physics instructors make when considering the qualities of a good general textbook. Reliability and validity of results from a questionary depend primarily on the adequacy of the questionary itself and on the number of answers obtained from a representative group.

Two copies of the questionary were sent to each of 212 colleges and universities with a request that one copy be filled out by the physicist in charge of the beginning course for technical students, the other by the one in charge of the course for nontechnical students. The institutions canvassed included: all state universities, all state colleges, and other colleges and universities. At the end of eight weeks 102 (24 percent) responses had been received from 69 (32.5 percent) colleges in 32 states. Among those who responded were several authors of physics textbooks. Many helpful comments were also received. This response was considered adequate and gratifying in view of the heavy responsibilities which most physicists are now carrying.

## The Questionary and Numerical Results

For purposes of presentation the questionary is reproduced here in a form more compressed than the original. Within it, composite results for all 102 respondents are given. Not all questions received 102 answers. The average number of answers per question was 96; the smallest number was 73. In order to save space the percentage answers to the *yes-no* questions are always given in parentheses and separated by solidi in the following order: (*yes*/*no*/*uncertain* or *either*). For example, 27 percent *yes*, 70 percent *no*, and 3 percent *uncertain* is written (27/70/3). For items that were answered by checking one of several suggested answers, the percentage of respondents who checked a given answer is indicated by a single number following the answer. All percentages are stated to the closest whole number.

The reader may be inclined to ask what conclusions may be drawn about the collective opinion of all general physics instructors on the basis of results obtained from only 102 of them. One must recognize at once the possibility that a selection factor operates in sampling by questionary; those who respond to the questionary may not be completely representative of the population being sampled. Unfortunately, there is no feasible way of detecting or estimating this difference if it exists. In the remainder of this paper it will be assumed that, as a group, those who replied to the questionary are in every significant respect representative of the whole group of physicists who teach general physics in colleges and universities.

The limited number of responses restricts the precision of the general deductions that can be

TABLE I. Significance of percentages.

| Affirmative percentage in this sampling | Range in affirmative percentages within which the true value probably lies |
|---|---|
| 90 | 84–96 |
| 70 | 61–79 |
| 60 | 50–70 |
| 50 | 40–60 |
| 40 | 30–50 |
| 30 | 21–39 |
| 10 | 4–16 |

made even on this assumption, but to an extent that can be calculated statistically. Estimates of the magnitude of this effect for the answers to the *yes-no*, or check, questions may be obtained by the use of well-known statistical formulas. The standard errors in the percentages of affirmative answers to such questions were computed. Now there is only one chance in 20 that the affirmative percentage for all physics instructors lies beyond a value which differs by more than twice the standard error from the affirmative percentage obtained here. On this basis, Table I was constructed. It indicates the reliability of the sampling process. It is practically certain (the probability is 0.95) that the affirmative percentage of all physics instructors is within the range given for a particular affirmative percentage obtained here. As an example, suppose there were a 40 percent affirmative answer to a question from 102 instructors; then the probability is 19/20 that between 30 and 50 percent of all general physics instructors hold an affirmative opinion.

It is evident from Table I that at least 60 percent affirmative answers in 102 replies are required to conclude that the majority of all general physics instructors are in the affirmative. An affirmative response smaller than 40 percent indicates that only a minority are in the affirmative. And from any response lying between 40 and 60 percent, no reliable majority opinion can be deduced.

In analyzing the response the answers were tabulated with regard to: (1) type of educational institution (liberal arts college, state university, state college, technical institution, and teachers college); (2) size of institution (less than 2000, 2000–5000, and more than 5000 students); and (3) type of course (technical or nontechnical). Courses were called technical if (*a*) the enrolment of engineering students and majors in physics, chemistry and mathematics equaled or exceeded 50 percent, and/or (*b*) the instructor stated the course to be technical. Otherwise courses were called nontechnical. Of the responses, 51 percent were for technical courses; 49 percent, for nontechnical courses.

The distribution of answers turned out to be independent of the type and size of educational institution, but was found in some cases to be significantly affected by the type of course under consideration. The questions whose answers revealed different opinions or data for technical and nontechnical courses are marked with an asterisk. They will be discussed later on the basis of the criterions of reliability set up in the next paragraph.

Although one could, with some assurance, judge intuitively which difference in percentage response for the two types of courses was significant, the significant differences in affirmative percentages were computed by common statistical formulas. Calculations were made for 50 respondents for technical courses and 50 for nontechnical courses on the basis that there be only one chance in 20 that such differences would occur by pure chance. Table II gives the ranges in percentages within which differences are not significant statistically. Whenever an affirmative percentage for one group of instructors lies outside the range given for a certain affirmative percentage for the other group, it is practically certain that instructors in the first group hold a collective opinion different from that of instructors in the second group. As an example of the use of Table II, suppose a 70 percent affirmative response is obtained from the technical course instructors (group 1); then there is only one chance in 20 that an affirmative response from the nontechnical instructors (group 2) will lie outside the range 51–86 percent. If it does, the two groups almost certainly hold a different average opinion.

## Questionary on General Physics Textbooks[1, 2]

*Background Questions on Your Course*

1. Your name. —— 2. Your title. ——
3. Your college or university. ——
4. Your college is: liberal arts college, 40%; state university, 31%; state college, 15%; technical institution, 11%; teachers college, 3%.
5. Approximate prewar enrolment in your institution: less than 2000, 44%; 2000–5000, 37%; more than 5000, 19%.

[1] See "Discussion of Answers" for results on items 8, 9, 11, 19, 20, 24, 42 and 48.

[2] Significant difference appeared in item 21 for only two parts: metric absolute and practical electric units. See "Discussion of Answers" for these two results.

6. Class hours per week in your general physics course are distributed as follows (average values given here): demonstration lecture, 1.8; recitation, 1.9; laboratory, 2.7.
7. Duration of your general physics course in weeks is: 35.6 (average).
8.* Prerequisites are: college algebra ——; trigonometry ——; calculus ——.
9.* Corequisites are: college algebra ——; trigonometry ——; calculus ——.
10. Approximate prewar enrolment in your general physics course: 16,700 (total).
11.* The students in the course are approximately ——% engineering; ——% liberal arts; ——% physics majors; ——% chemistry and mathematics majors; ——% premedical. Is there any other large fraction of students? If so, name ——, ——%.
12. The students in the course are approximately: 22% freshmen; 60% sophomores.
13. The physics course is required of about 69% of the students in the course.

*General Questions on Content*

14. Approximately what percentage of a textbook should be devoted to the following major topics? Mechanics, 25; heat, 13; electricity and magnetism, 23; acoustics, 7; optics, 14; modern physics, 5; other topics, 0. (Average values given.)
15. Should a chapter on the history of physics be included? (41/58/1) If so, should it be at: the beginning? 27%; the end? 43%; elsewhere? 30%. (Many stated that history should be scattered throughout the text.)
16. Should a chapter reviewing requisite mathematics be included? (43/57) If so, should it be at: the beginning? 53%; the end? 22%; elsewhere? 25%. (Many suggested the appendix.)
17. Should a section on the use of the slide rule be included? (26/74) If so, should it be at: the beginning? 60%; the end? 12%; elsewhere? 28%.
18. Should a section on experimental error be included? (46/54) If so, should it be at: the beginning? 62%; the end? 5%; elsewhere? 33%.
19.* Should trigonometry be used: extensively? —— occasionally? —— never? ——.
20.* Should calculus be used: extensively? —— occasionally? —— never? ——.

TABLE II. Significance of difference in affirmative percentages from two groups of respondents.

| Affirmative percentage in sampling from group 1 | Range in affirmative percentages for group 2 which are not significantly different from group 1 |
|---|---|
| 90 | 78–96 |
| 70 | 51–86 |
| 50 | 31–69 |
| 30 | 14–49 |
| 10 | 4–22 |

21.* Which systems of units should be developed? British gravitational, 79%; British absolute, 34%; metric gravitational, 37%; metric absolute, ——; meter-kilogram-second, 38%; electromagnetic, 52%; electrostatic, 61%; practical electric, ——; other, 0%.
22. Which systems of units should be used in problems? British gravitational, 80%; British absolute, 28%; metric gravitational, 30%; metric absolute, 79%; meter-kilogram-second, 34%; electromagnetic, 43%; electrostatic, 50%; practical electric, 79%; other, 0%.
23. Should formulas be used if not derived? Usually? 9%; occasionally? 78%; never? 13%.
24.* Do most textbooks offer enough material for a follow-up course?
25. Should a textbook include enough material for the above purpose? (29/71)

*Specific Questions on Content*

26. Do you favor the use of "atomic mass" and "molecular mass" in place of "atomic weight" and "molecular weight"? (59/35/6)
27. Would mentioning centripetal force, and never centrifugal force, be desirable? (30/68/2)
28. Some textbooks correctly define speed and velocity and then occasionally use them as synonyms. Is the distinction worth continued emphasis? (66/33/1)
29. Do you prefer to define the specific heat of a substance as being (i) "The quantity of heat which must be given to (or taken from) a unit mass of that substance to increase (or decrease) its temperature one unit" as compared with (ii) "The ratio of the thermal capacity of the substance to that of water"? ((ii)70/(i) 28/2)
30. Is it desirable to have a treatment of alternating-current circuits comparable in scope to that of direct-current circuits? (35/64/1)
31. Do you favor a treatment of magnetism that avoids the concept of magnetic poles? (19/80/1)
32. Do you favor including a section on simple radio theory? (87/13)
33. Should a discussion of musical scales be omitted? (42/58)
34. Should a fairly complete discussion of loudness-level contours and hearing be included? (63/37)
35. Do you favor emphasis on physical optics? 12%; on geometric optics? 11%; equal weight? 76%.
36. Should the formula $X\cdot X'=F^2$ be developed and used for curved mirrors and lenses? (23/77) If so, should this formula displace the reciprocal formula $(1/p)+(1/q)=1/f$? (18/82)
37. Is there need for a more extended treatment of the relation between magnification and resolving power for microscopes and telescopes than most physics textbooks give? (52/48)
38. Which commonly included topics, not already mentioned, should be omitted? 52 topics were listed. Topics listed four times were polyphase currents, precession and the gyroscope. Listed three times were types of batteries. Listed twice were thermoelectric effect, telegraphy, relativity, electrostatic details, polarized light and a mathematical statement of the Bernoulli theorem.

39. Which commonly omitted topics, not already mentioned, should be included? Of 41 topics listed, meteorology was listed ten times; photography, three times. Topics listed twice were dimensional analysis, airplane dynamics, Kepler's laws, more use of vector notation and more discussion of elasticity.
40. List topics (no more than three) which you think textbooks often present incorrectly, and, for each topic, give the name of a textbook that treats it correctly. Here 38 topics were given. Newton's third law was given seven times; permeability, four times. Topics listed three times were: units in mechanics, siphon, Bernoulli theorem. Those listed twice were: rotary motion, gyroscope, magnetic circuit, mass and weight, electromotive force, $B$ and $H$ in magnetic materials. (Few textbooks were named.)

*Questions on Order of Content*

41. List your preferred order of major topics (mechanics, heat, acoustics, electricity and magnetism, optics and modern physics): MHAEO, 25%; MHAOEMp, 23%; MHEAOMp, 18%; MAHEO, 16%; assorted orders, 18%.
42.* Should kinematics precede dynamics?
43. Should treatment of statics precede dynamics? (69/30/1)
44. Should treatment of the simpler concepts of the mechanics of fluids precede mechanics of solid objects? (20/76/4)
45. Should electromagnetism and magnetism follow electrostatics and current electricity? (55/43/2)

*Pedagogy*

46. Is it your philosophy that it is better for the majority of students to (i) learn a few important points thoroughly, or (ii) have a general, but less thorough, knowledge of the whole subject? [(i)66/(ii)30/uncertain 4]
47. Should force be introduced as (i) an intuitive concept or (ii) as a derivation from Newton's laws? [(i)54/(ii)35/"both" 11]
48.* Is it necessary always to preserve a strictly logical order?
49. Should descriptions of applications of concepts usually precede the theory? (53/46/1)
50. Should the textbook tend to utmost mathematical rigor within the scope of the student? (68/32)
51. Should chapter summaries be included? (73/26/1)
52. Should answers be given to the problems? All, 28%; half, 68%; none, 4%. At end of problem, 50%; at end of book, 50%; in answer book obtainable by student with instructor's permission, 0%.
53. How many problems are sufficient for a typical chapter? 17.8 (average).
54. Should data for problems nearly always be chosen to provide easy mathematical manipulation? (56/41/3)
55. Should line diagrams be used? Always, 6%; often, 89%; occasionally, 5%; never, 0%.
56. Should half-tone illustrations be used? Always, 0%; often, 30%; occasionally, 69%; never, 1%.
57. Are a few full-page diagrams or pictures more valuable than a larger number of smaller ones taking up the same area? (7/86/7)
58. Do you favor the mildly comical illustrations appearing in some recent textbooks? (39/61)
59. Should the textbooks usually describe demonstration experiments likely to be performed during lectures? (60/39/1)
60. List topics (no more than three) which you think textbooks often present poorly from the pedagogic point of view and, for each topic, give the name of a text that treats it well. Of 33 topics given: those listed three times were Newton's second law, units on mechanics; those mentioned twice were mass and weight, the Bernoulli theorem, alternating currents, simple harmonic motion. (Few textbooks were named.)

## Discussion of Answers

Answers that seem to merit discussion, beyond the percentages already given, will be dealt with approximately in the order in which they appear in the questionary. Some deductions will be given. These deductions are, of course, based on the results from the questionary and not on the personal views of the author.

The first generalization obtained from the original tabulation of answers was that there is no difference of majority opinion on desirable textbook characteristics among responding instructors in educational institutions of different types and sizes. It is true that, since the number of responses in any of these classifications did not exceed 45, a statistically significant difference requires a rather large percentage difference in response. Even so, the agreement in response that was noted from these classifications was remarkably close. This agreement seems to indicate that any broad differences in characteristics between educational institutions, due to variations in size and type, are not reflected in majority recognition by physics instructors of a need for marked differences in general physics textbooks used by them.

Apparently the percentages of most modern textbooks (item 14) devoted to the different major fields of physics are in agreement with instructor opinion. Or, have instructors been conditioned to accept these proportions as proper? The several orders of topics in present physics textbooks are fairly satisfactory to instructors, according to question 41. A large majority agree that mechanics should be taught first. Perhaps other answers might have been obtained from instructors if many of them had tried giving general physics courses in which the relative

fractions and orders of the topics had been greatly changed from the few commonly used.

Of the nine questions showing significant differences in the percentage response from those teaching technical courses and those teaching nontechnical courses, four are so crucial in the make-up of a text that they dictate the entire attitude of the book and the presentation of topics therein. But the answers to these four questions need not affect the choice of topics. In fact, the answers to the remainder of the questionary show that the list of topics need not differ between texts designed for the technical student and those for the nontechnical student. The four questions are items 8, 9, 19 and 20. Technical courses have prerequisites: college algebra, 69%; trigonometry, 78%; calculus, 6%. Their corequisites are: college algebra, 12%; trigonometry, 22%; calculus, 37%. The instructors in technical courses agree that trigonometry should be used: extensively, 78%; occasionally, 22%. Calculus should be used: extensively, 16%; occasionally, 62%; never, 22%. These results indicate strongly that relevant algebra and trigonometry may be used as fully as necessary. Calculus methods may be hinted at and may even be often used for texts designed for technical courses in about 40% of the colleges. This extended use of elementary mathematics should, of course, affect the presentation in the entire textbook.

Nontechnical courses have prerequisites: college algebra, 42%; trigonometry, 46%; calculus, 0%. Corequisites are: college algebra, 27%; trigonometry, 27%; calculus, 2%. Instructors state that trigonometry should be used: extensively, 25%; occasionally, 75%. Calculus should be employed: extensively, 0%; occasionally, 28%; never, 72%. A nontechnical text, therefore, may and should use elementary trigonometry but never calculus. This will, of course, make a nontechnical text less mathematical in actuality and in appearance.

The answers on units of measurement (items 21 and 22) indicate that units which are used should be developed and *vice versa*. British absolute and metric gravitational units may well be dropped. Apparently the mks system has not made much headway even though its adoption has advantages. Perhaps we, as teachers, feel that it is just another set of units to add to the confusion. The opinion on units shows significant differences only for the development of metric absolute and practical electric units. The technical course instructors favored the metric absolute units 92 percent as compared with 69 percent for the nontechnical courses; while technical course instructors favored the practical electric units 73 percent as compared with 92 percent for the nontechnical course.

The matter of policy in item 23 is often discussed by instructors, and it appears that a large majority favor the occasional use of formulas which are not derived. Conversely, very few are in favor of deriving all the formulas or in deriving none. Presumably, the formulas not to be derived are the more complex ones. The technical and nontechnical course instructors are in very close agreement here. The largest difference was only 2 percent. This, of course, cannot mean that these instructors agree on which formulas are to be derived. Since textbooks for nontechnical courses are restricted to the use of algebra and rudimentary trigonometry, the formulas used in them are of necessity fewer in number and simpler.

There is significant difference of opinion on item 24. The technical course instructors agree 70 percent that there is not enough material in most textbooks for a follow-up course. The nontechnical course instructors are evenly divided on this, with 44 percent *yes* and 56 percent *no*. Both groups agree emphatically that a text should not attempt to offer follow-up material.

Items 26, 28 and 29 are typical of some questions dealing with terminology. Would not instructors agree that a physics course should teach the student directly, and by example, the need for and advantages of exact, unique meanings of words and expressions? However, barely more than half of the instructors favor the use of "atomic mass," which is less ambiguous than "atomic weight" if strict technical accuracy is sought. About a third of the physics instructors think the distinction between speed and velocity unimportant in elementary textbooks.

A majority of instructors are opposed to a treatment that avoids the concept of magnetic poles (item 31). Presumably, the analogy between electrostatics and magnetism on this basis is considered desirable pedagogically. It is, however, possible to discuss magnetism completely without ever introducing the idea of a magnetic pole as an entity. Might this prove to be just as understandable by the student as the theory now presented? This question has a bearing on item 45. If the magnetic pole concept is not used, electromagnetism and magnetism, in that order should follow current electricity.

Physics instructors are opposed to the use of the formula $X \cdot X' = F^2$ for lenses, lens systems and mirrors (item 36). Yet it can be as easily developed geometrically as the reciprocal formula. Advantages often claimed for it are: easier arithmetical work; easier determination in the laboratory of the positions of the principal planes for optical systems.

One might expect a marked difference in answers to questions 38, 39 and 40 from technical and nontechnical course instructors, but this was not the case here. There must be many topics in existing textbooks that are favorites of some professors and definitely out of favor with others since a number of topics appeared in answer to both questions 38 and 39.

There is a barely significant difference in opinion as to whether kinematics should precede dynamics (item 42). The nontechnical course instructors favor (70 percent) the order given; the technical instructors strongly favor (84 percent) the order given. The composite result is (77/20/3).

The response to item 46 is interesting, for it shows that theory and practice are in sharp conflict. Sixty-six percent of the respondents think it better to design a physics course so that a majority of the students learn a few points thoroughly. This figure indicates that a majority of physics instructors hold this view. (Technical course instructors favor it strongly, 70 percent.) Yet, so far as the author knows, no textbook has this aim and few, if any, courses are taught on this basis. It must be obvious to any instructor that the majority of students retain only a small fraction of the knowledge presented in our 700- to 800-page textbooks. It is true, of course, that a textbook must contain enough material to allow for some difference between courses in which it is used. Even so, a strong case can be made for drastically reducing the number of topics included and for presenting these more thoroughly. However, this conclusion for answer 46 seems to be partially contradicted in the response to question 38, in which only 52 topics were listed for deletion. Only nine of the topics were listed by more than one instructor, and many did not suggest any deletion.

Item 48 indicates that most instructors think it unnecessary always to present topics in a strictly logical order. Only 7 percent of the nontechnical course instructors favor strictly logical presentation, while 30 percent of the technical course instructors favor this method. The composite result is (19/80/1). Item 47 is related to item 48. Most instructors think that force should not be introduced logically as a derivation from Newton's laws, but that it should be introduced as an intuitive concept, or in both ways.

Finally, there is the question: to whom are the first courses in physics being taught? Item 11 shows that for the technical courses there are 63 percent engineering students, 6 percent liberal arts students, 4 percent physics majors, 8 percent chemistry and mathematics majors, and 8 percent premedical students. In the nontechnical courses there are 7 percent engineering students, 28 percent liberal arts students, 3 percent physics majors, 9 percent chemistry and mathematics majors, and 33 percent premedical students. Average percentages are given. Physics textbooks evidently cannot be designed for physics majors alone since they represent the smallest of the five groups of students listed.

In conclusion, the results indicate that physics instructors agree well on desirable major characteristics of general physics textbooks. They favor present policies of most authors in choice and presentation of major topics. There is no unanimity of opinion on choice and method of presentation of many minor topics. Present policies of most authors on the use of mathematics and formulas find support among the instructors. There seems to be a need for a textbook written with the idea of presenting thoroughly a rather small number of important topics since about two-thirds of the physics instructors think it better to offer physics courses on this basis.

The writer appreciates the kindness and interest of the physicists who responded to the questionary. He is also indebted to several of his colleagues for helpful discussion and criticism of the questionary and this paper.

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*TRUE opinions can prevail only if the facts to which they refer are known; if they are not known, false ideas are just as effective as true ones, if not a little more effective.*

—Walter Lippmann.

# A Proposed Reorganization of Undergraduate Physics

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ONE of the most difficult problems facing teachers in a rapidly advancing field such as physics is to evaluate the new knowledge and to put it in its proper place within the college curriculum. It is a task that requires and merits the organized attention of the entire professional group.

The usual tendency is to try to lump all the new knowledge into one or more new courses, with as little as possible disturbance of the courses already being given. Although this solution has many weaknesses, it is simple, and satisfactory for a limited time. Thus the curriculum grows until there are so many courses that the student for lack of time must omit some of the basic ones and so leave gaps in his training.

Similarly, in a rapidly expanding industrial world, there is a tendency in colleges to add a large number of courses designed to teach specialized technics, such as radio, x-ray technology, photography and the like. After the war, aviation physics, television and radar will no doubt join the list. Such courses are often given in departments that do not have an adequate staff for even the basic courses in physics. Where the staff is large enough to teach these courses in addition to the fundamental ones, there is a tendency to allow the student to substitute them for the basic ones. Although he undoubtedly learns some fundamental physical principles in these courses, they are designed primarily to give him a mastery of the technics rather than an understanding of natural laws. We think they are more suitably termed physical engineering than physics.

There should be an effort to prevent physics from breaking up into a hodgepodge of engineering technics at the expense of depriving the student of the power that comes from a study of the general physical principles underlying all applications now in use as well as many to be developed, and the joy and satisfaction that come from understanding the structure and workings of nature. Practical applications should be liberally distributed through all courses, but these should be used to clarify general principles rather than to develop specialized skills. Thus a radio engineer should study radiation and electricity to understand radio; a physicist should study radio to help him understand radiation and electricity. If this distinction is not made, it would seem that engineering and physics become identical in purpose.

We believe the time has come for a thorough reorganization and nation-wide standardization of the undergraduate curriculum in physics, leaving out enough of the less important material and the specialized industrial applications so that the pure physics can be taught in a limited number of basic courses to the end that the undergraduate majoring in physics may have the best possible training in the field and not be intellectually cheated of part of his basic training. Several advantages immediately follow from the adoption of a standardized undergraduate course of study. Among other things, it will tend to raise the level of undergraduate instruction. It will make a bachelor's degree in physics come to have a definite meaning both to prospective employers and to those in charge of graduate schools and research institutions. It will mean that a graduate of any standardized undergraduate department of physics can enter any graduate school on even terms with men from other institutions, and the graduate programs of study can be organized to better advantage. On the other hand, no scheme for standardization should be adopted that does not make adequate provision for keeping the standard curriculum fully abreast of advances in the subject.

In what follows we are suggesting a regrouping of the topics we feel should be covered in undergraduate physics, with names for the reorganized courses. This grouping is not to be regarded as in any sense the final word on the subject, but it is offered as a basis for study and discussion. We are also suggesting a procedure for bringing about standardization.

## Suggested Program for an Undergraduate Major in Physics

*First Year*

**I. General Physics.** One year; 8 semester hours.

Elements of mechanics; properties of matter; heat; sound; electricity; light; selected topics in so-called modern

* On leave for war work from the University of Oregon and Mary Hardin-Baylor College, respectively.

physics, such as elements of electronics, radio, x-rays, radioactivity.

To be preceded or accompanied by advanced algebra and trigonometry.

*Second Year*

**II. Chemical Physics.** First semester; 4 semester hours.

Atomic and molecular physics; introduction to nuclear physics; physics of solids; rheology; heat and thermodynamics.

To be preceded or accompanied by general chemistry and elementary calculus.

**III. Mechanical Physics.**

Second semester; 4 semester hours.

Analyses of forces and moments; principles of conservation of energy and momentum; dynamics of linear and rotary motion; vibratory motion; inverse-square-law fields of force.

To be preceded or accompanied by elementary calculus.

*Third Year*

**IV. Electrophysics.** One year; 6 to 8 semester hours.

Electrostatics; electrolytics; direct and alternating currents; electromagnetism; electronics, including thermionic and photoelectric emissions, modern vacuum and gas-filled electronic tubes, introduction to circuits; technics and theory of ultra-high frequencies, including velocity modulation of electron beams, cavity resonators, etc.; high voltage generators—the Van der Graaff machine and the cyclotron; the electron, proton, deuteron, positron and other electric particles usually considered in courses in modern or nuclear physics.

*Fourth Year*

**V. Radiation Physics.** One year; 6 to 8 semester hours.

Wave motion and sound, including supersonic radiation; electromagnetic radiation, including the wave and quantum properties of radio waves, microwaves, infra-red, visible and ultraviolet radiations, x-rays and gamma-rays, also such applications of these properties as spectroscopy and x-ray diffraction; electron optics, including the principles of focusing and directing electron beams such as are used in the electron microscope, iconoscope and other cathode-ray tubes; the wave properties of cathode rays.

Each of the proposed courses should require at least 12 clock-hours of the student's time per week to achieve the credits indicated in the outline, and should consist of lectures, recitations and laboratory, carefully integrated and in such proportions as may seem best in the individual institution. It is assumed that considerable latitude will be exercised by the individual instructor in his treatment of any particular topic.

Though the courses are outlined for a four-year program, they could easily be fitted into a three-year period. If a physics major should take Course I in his second year, he could take II and III in his third year and IV and V, in parallel, during his fourth year.

It will be noted that the names of the courses beyond that for the first year differ from those usually seen in college catalogs. We have deliberately included "physics" in the title of each course. The inclination in the past to omit "physics" in course titles and in specialized fields has caused students and general public alike to fail to learn what constitutes physics. Admittedly, the word is a tongue-twister and is frequently confused with calisthenics or the medical profession; but it should be associated with the subject or else a new word should be chosen. We physicists could learn much from our close neighbors, the chemists, who have enjoyed a good measure of public recognition. They have associated their undergraduate courses with their professional fields, and with both the words "chemistry" and "chemist." Thus they have a course in physical chemistry, and have physical chemists; a course in organic chemistry, and a specialized field of organic chemistry; inorganic chemistry, and inorganic chemists; analytical chemistry, and specialists known as analytical chemists. They even have chemical engineers! The undergraduate courses in physics, by contrast, are usually given such names as optics, heat, mechanics and electricity, which only to the initiated suggest any relation with physics. And whoever heard of a physical engineer? It is not surprising that news commentators and the general public are reluctant to use the word when physicists themselves shy away from it.

A few comments may help to clarify the proposed outline. *General physics* is the usual beginning course in college physics revised to suit the times. It should serve the dual purpose of laying an adequate foundation for the more advanced undergraduate courses and of providing a suitable introduction to physics for students who will take only the one course.

*Mechanical physics* is the usual undergraduate course in theoretical mechanics accompanied by laboratory experiments.

Atomic and molecular physics as presented in *Chemical physics* would not deal with chemical properties any more than necessary; but it would attempt to familiarize the student with such physical characteristics of atoms as atomic numbers, masses, and radii; the nature and magnitude of forces between atoms and between molecules; dipole moments; magnetic suscepti-

bilities; interatomic distances; bond angles; the physical properties of high polymers; the structure of simple crystals. It would not give detailed consideration to such technics of determining atomic and molecular structure as spectroscopy, x-ray and electron diffraction, which would be taken up in Course V. The section on nuclear physics would be confined to a description of qualitative results and would not go into the experimental technics, which would be studied in Course IV.

*Electrophysics* would combine elements of the traditional course in electricity and magnetism—excepting electromagnetic radiation—with the more recent aspects of the physics of electricity, now usually thought of as electronics or as modern physics. It would emphasize the particle point of view of electricity and would include a study of the electrical theory of matter.

*Radiation physics* would include a study of the nature and properties of electromagnetic radiations, as well as the technics of controlling and using them. It would contain an introduction to Maxwell's equations and the quantum theory. In addition to topics found in the usual course in optics, it would offer a study of antennas and reflectors for radio and microwaves, of wave guides, and of the optical technics of x-ray, infra-red and other electromagnetic radiation not in the visible region. It would not deal explicitly with the technics of production of these radiations, topics included in prior courses, but would deal to some extent with the interaction of radiation with matter.

In all these courses the experimental, descriptive and theoretical aspects of the subjects should be interwoven wherever they logically occur. We think that the practice of teaching experimental and theoretical physics in separate courses is illogical and undesirable, particularly on the undergraduate level. Practice in the skills a physicist needs, such as the use of various tools, especially machine tools, soldering, glass blowing and photography, should be taught either in a separate course or given as special instruction in the laboratory exercises of the regular courses.

In designing this program we have tried to integrate topics which have common fundamental principles but which for various reasons have been taught as separate courses. Probably the most striking achievement of modern physics from a philosophic or a pedagogic point of view is the tunneling through far beneath the surface of things to a genuine unity in physical phenomena, a unity based on experimental facts rather than on philosophic speculation. Yet physics teachers as a group have not exploited this advantage. The principal undergraduate courses are still organized according to the 19th century pattern, which frequently cuts across the lines of this unity. The new physics has not been allowed to influence to a significant extent the organization of the basic courses. It has been more or less tacked on to the undergraduate curriculum either in the form of separate courses or separate chapters miscellaneously attached to the texts of the older courses. For this reason it has tended to divide rather than to unify physics. In addition to, and partly as a result of, this failure to lay sufficient stress on the coherence of physics, there is a detrimental lack of uniformity in the training received by physics majors in different institutions. There is a disturbing tendency for physics to separate into discrete fields with no common name and with the different specialists, particularly those who go into industry, not feeling the coordinating force of a common background.

The American Institute of Physics has done much to prevent the disintegration of physics by bringing together the separate organizations within the broad field. It could serve the cause further by cooperating with the American Association of Physics Teachers in sponsoring a program for an undergraduate major in physics which would be designed to emphasize the unity of the underlying principles of physics and which would be to some extent common to various institutions. For example, a standardization committee might be organized with, say, 15 members representing the five organizations incorporated in the Institute of Physics, elected, or appointed, in three classes with each member serving for, say, six years. This committee would prepare a "standard undergraduate course in physics" and keep it revised to date. The standard course would be helpful to curriculum organizers and textbook writers throughout the country. Collegiate institutions would gradually come to adopt it, in whole or in part, simply because of its obvious merits.

# Giving Power to Words

PHILIP W. SWAIN
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"FOOLS rush in where angels fear to tread." Perhaps that is why I am not averse to giving physicists advice on how to write and edit. Within the past year I have told school mathematics teachers how they should teach, engineering deans what they should do about English studies, mechanical engineering professors what subjects they should teach and how.

Now I am at it again. Sooner or later this sort of thing may lead to my public undoing by some real expert.

So let me disarm you with a bit of frankness. I am not an expert writer. I feel like one of my engineering students at Yale many years ago. He wrote his "no help" pledge this way on his examination paper: "Mr. Swain, I give you my word of honor that I have neither given nor received help in this examination, of which the foregoing pages are ample evidence."

Aside from an invitation to do so, my only excuse for writing this paper is that I have been struggling with the practical problems of technical writing and editing for 23 years, and came to the editor's job with a rather broad background that included a lot of Greek and Latin study, an academic major in physics, tutoring in physics, industrial factory jobs as machinist and steam fitter, two degrees in mechanical engineering, teaching and practicing mechanical engineering, serving as an army artillery officer and as a salesman.

Today I am convinced of what I long ago suspected—that both physics and English are superlatively useful studies. Physics, the most fundamental of all engineering fundamentals, was my first love, and is still my favorite study.

Your work as teachers of physics in an age of applied science is of first importance. To do your job right you will need skill with three languages: the language of words, the language of mathematics and the language of pictures. Here we talk about the words.

## KNOW YOUR READER AND YOUR OBJECTIVE

The first rule of good writing and good editing is to know your reader—who he is, how he thinks; what words he uses, what words he understands; what interests him, what bores him; wherein he is smart, wherein stupid.

The second rule is to know what you wish to accomplish in the way of giving pleasure, causing amusement, winning friendship, imparting knowledge, awakening interests and reforming attitudes.

You are physicists and teachers of physics, so I assume that you write mostly for other physicists, for scientists in related fields, for students of elementary physics, for students of advanced physics and for the nonscientific public. More often than not, I assume, your aim will be to impart some understanding of physical laws, facts, methods or points of view, together with some share of your own enthusiasm.

So you physicist writers start by knowing just what you want to do, and to whom. There remains only "How?," the sixty-four dollar question. It is a question I cannot answer, nor anybody else. I can only pass along a few tips, and urge you to build skill by constant writing, testing and rewriting.

It will not be easy. English, the most gloriously illogical major language, is better for poets than for physicists. But you will always find the right word if you look long enough.

## Many Languages

Note the many languages within our language. The college freshman learns that "the moment of a force about any specified axis is the product of the force and the perpendicular distance from the axis to the line of action of the force." Viewing the same physical principle, the engineer says: "To lift a heavy weight with a lever, a man should apply his strength to the end of a long lever arm and work the weight on a short lever arm." Out on the factory floor the foreman shouts, "Shove that brick up snug under the crowbar and get a good purchase; the crate is heavy." The salesman says: "Why let your men kill themselves heaving those boxes all day long? The job's easy with this new long-handled pinch bar. With today's high wages you'll save the cost the first afternoon."

Who can say which of these is the best English, or the worst? Each seems well suited to its purpose and audience, therefore good English, according to my lights.

May I venture to suggest a useful writing

exercise for grown-up physicists. First write a sedate and technical little treatise on the gyroscope—say, 1000 words. You might entitle it "A concise summary of the physical principles underlying gyroscopic phenomena." Then rewrite (and retitle) the piece seven times for the following seven types of audience or reader:

The Latin faculty of your university.
Some imaginary university president to be impressed by your profundity.
A student of first-year physics.
The *Reader's Digest*.
A Rotary Club meeting.
A mechanic, skilled but unschooled.
An eager 10-year-old boy, interested in gadgets.

### TEN "RULES"

Now, at the risk of appearing utterly inconsistent, I am going to set down 10 "rules" of good writing. It will be fair to ask me how there can be general rules if it is true that each type of reader requires special treatment. Somewhat lamely, I answer that certain *ways* of writing (as distinguished from content) seem to go over well with the majority of American readers of all classes. If you follow these 10 suggestions blindly you will not always be right, but you will be right more often than you are wrong:

1. Short words are better than long.
2. Short sentences are better than long.
3. Short paragraphs are better than long.
4. Short articles are better than long.
5. Direct statement is better than indirect.
6. The active mood is better than the passive.
7. Don't pussyfoot.
8. Be simple, human and concise—not complex, pompous and verbose.
9. Don't overwork "is," "was" and other parts of "to be."
10. To gain power, chop off Latin roots wherever Anglo-Saxon words can tell the same story. Thus, "They *considered* it *improbable* that *circumstances* would *permit* him to *divulge* the *occurrence*," would better be, "They didn't think he had a chance to tell the news."

Thus baldly set down without a lot of justifying "ifs" and "buts," these so-called rules may expose me to a barrage of exceptions from literary and scientific people. I grant the exceptions, but still insist that those who can write by these "rules" when they want to can best be trusted to break them wisely.

I must not appear to criticize the writings of one trained physicist to another, or the language these two understand best even if some words are both long and Latin.[1] Two men of one mind talking shop are a pretty sight, whether they be physicists, machinists or baseball fans. They understand each other, and that is all that counts.

But let me ask you how many physicists can go through life with no fair opportunity to instruct or inspire some of the 99.997 percent of Americans who are not physicists. If we believe in the power and glory of physics, can we justify wasting such opportunities or use the lame excuse that the other fellow is stupid?

Learning to talk and write so the other fellow can understand you is a prime duty of every man with any wisdom worth sharing. The rules I have listed apply with particular force wherever the subject discussed is strange to the reader.

Whether you agree or not, I urge you to try this experiment: Hunt up some bit of expository writing full of long words, Latin roots, long sentences, long paragraphs and the passive mood. Rewrite by the "rules," and test the result. Is it easier to read? Does it save the reader's time? Does the writing have more power? What scientific or literary values, if any, have been lost?

### Shakespeare Rewritten

Out of curiosity, I reversed this procedure to see if it would make good writing bad. It did. My starting point was part of Mark Antony's speech to the mob in *Julius Caesar*. Rome might seem a good place to use Latin words, but Shakespeare knew better and made Mark speak Anglo-Saxon because there was a job to be done and no time for fooling around. Note also that Mark Antony shows no fear of direct statements, personal pronouns or one-syllable words. I now quote as Shakespeare wrote:

Good friends, sweet friends, let me not stir you up
To such a sudden flood of mutiny.
They that have done this deed are honourable:
What private griefs they have, alas! I know not,
What made them do it; they are wise and honourable,
And will, no doubt, with reasons answer you.
I come not, friends, to steal away your hearts:
I am no orator, as Brutus is;
But, as you know me all, a plain blunt man,
That love my friends; and that they know full well
That gave me public leave to speak of him.
For I have neither wit, nor words, nor worth,
Action, nor utterance, nor the power of speech,
to stir men's blood; I only speak right on. . . .

[1] For an analysis of such writings, see D. Roller, Am. J. Physics **13**, 99 (1945).

Now I will rewrite into this speech the faults of the typical engineering manuscript—long words, long sentences, pussyfooting expressions, passive moods, failure to call the other fellow "you" and yourself "I." Note the difference!

It is not the intention of the speaker to create in the minds of the friends and other gentlemen present any rapid increase in antagonistic and violent emotions. The persons who sustain the responsibility for this action are gentlemen of substantial reputation. It has not been feasible for the speaker to determine what personal grievances may have impelled them to concur in the action under discussion. However, due to the fact that they are intelligent and of satisfactory reputation, it may be assumed that they will stand prepared to present apparently defensible explanations of their procedure.

It should not be considered to be the intention of the speaker in appearing before you to influence your emotions in such a way as to advance his own personal selfish interests. The speaker is not properly what might be termed an adept in the profession of public speaking, as might be properly stated of Mr. Brutus. It is perhaps not unreasonable to make the assumption that all of the gentlemen here present are acquainted with the fact that the speaker is a person of uncomplicated character and one not addicted to circumlocutions and other types of round-about operations—also that an understandable fondness was maintained by the speaker toward this rather close acquaintance.

Circumstances such as these are entirely familiar to those gentlemen who have accorded the speaker permission to present comments regarding this person. As far as the personal qualifications of the speaker are concerned, his abilities do not include the intelligence, the vocabulary or the character, the procedure, the verbal delivery or the skilfulness in enunciation requisite to the creation of excessive excitement in an audience. To speak with entire accuracy, it is practically impossible for the speaker to accomplish more than an unpretentious enumeration of the circumstances of a situation in correct sequence.

**ENGLISH *VERSUS* JARGON**

Here is a typical engineer's sentence of 39 words, most of which are wasted.

It is assumed ordinarily that in computations of this character it is desirable to arrange the various elements of the problem in the form of a tabulation in order to insure that avoidable errors are reduced to a minimum.

Why not say:

Most engineers like to tabulate such data to reduce errors.

Lampooning the pompous jargon of doctors, lawyers, engineers and other professionals, C. M. Ripley, of the General Electric Company, quotes an economist:

The significance of this widespread problem can be grasped when it is recognized that the phenomenal growth of cities which has taken place during the past several decades is attributable to our efforts, in this industrial era, to achieve convenient physical accessibility between the many interdependent parts of our intricate economic and social mechanism.

The economist is trying to tell us that cities have grown because of good transit systems.

Ripley shows how short words bring power to the King James version of the Bible:

Rise up, my love, my fair one, and come away. For lo, the winter is past, the rain is over, and gone.

From all English literature before Shakespeare, Ripley reminds us, Bartlett lists 363 familiar quotations averaging 11 words each—and three words out of four have but one syllable.

**Conclusion**

In closing let me sum up in this way: (1) know your reader; (2) know your objective; (3) be simple, direct and concise.

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*THE men of culture are those who have had a passion for diffusing, for making prevail, for carrying from one end of society to the other, the best knowledge, the best ideas of their time: who have labored to divest knowledge of all that was harsh, uncouth, difficult, abstract, professional, exclusive; to humanize it, to make it efficient outside the clique of the cultivated and learned, yet still remaining the* best *knowledge and thought of the time, and a true source, therefore, of sweetness and light.*—MATTHEW ARNOLD.

# Coupled Pendulums: An Advanced Laboratory Experiment

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IN the study of oscillating systems many examples are found in which the interaction between the driving and the driven systems cannot be neglected. This is true whether driver and driven are two distinct systems or the driver is simply one mode of oscillation of a single system that is capable of vibrating in several different ways. Much insight into the fundamental physics of such systems can be secured through a quantitative experiment performed with very simple equipment.

Satisfactory apparatus consists of two small lead spheres attached to fish line strings so as to be supported as simple pendulums from a horizontal fish line, as shown in Fig. 1. It is convenient to pass one end of this string over a pulley so that the force in the string may be varied. It is easy to vary the length of one of the pendulums and also to alter their distance of separation on the horizontal string. This equipment is thus sufficiently flexible to allow the investigation of several aspects of the motion of coupled oscillators. It has been found that the coupling results from lateral motion of the supporting member. This is true even if a stiff wire is used for support and the pendulum strings or wires are rigidly attached to it. Such a situation is depicted in the "top view" of Fig. 1. The experiments discussed herein have been performed with a symmetrical system ($a=b$), and the angular amplitude of motion has been kept constant and small.

### Theory of Coupled Oscillators

The theory of coupled oscillators is ably presented in several books. A very satisfactory treatment is given by Morse,[1] and the present outline is based on it.

The differential equations of motion of the two oscillators are, assuming equal masses,

$$\begin{aligned} m\mathrm{d}^2x_1/\mathrm{d}t^2 &= -K_1x_1+K_3x_2, \\ m\mathrm{d}^2x_2/\mathrm{d}t^2 &= -K_2x_2+K_3x_1, \end{aligned} \tag{1}$$

where $K_3$ is the coupling coefficient, or force in dynes on oscillator *1* per unit displacement of oscillator *2* and *vice versa*.

Constants $\nu_1$, $\nu_2$ and $\mu$ are introduced as follows:

$$\begin{aligned} K_1 &= 4\pi^2\nu_1^2m, \\ K_2 &= 4\pi^2\nu_2^2m, \\ K_3 &= 4\pi^2\mu^2m; \end{aligned}$$

$\nu_1$ and $\nu_2$ are the free, uncoupled frequencies of vibration of oscillators *1* and *2*, respectively, and $\mu$ is a new coupling coefficient. The solutions of the differential equations of motion are of the form $x_1=\mathrm{f}_1(t)$ and $x_2=\mathrm{f}_2(t)$. These will in general be nonsinoidal and not even periodic. If it is demanded that the solution be simple harmonic, one finds that this condition can be satisfied provided the system is set in motion under proper conditions. For a pair of such coupled oscillators there are two ways in which the system will oscillate simple harmonically, and the frequencies $\nu_+$ and $\nu_-$ are, respectively, higher than and lower than either $\nu_1$ or $\nu_2$; $\nu_+$ and $\nu_-$ are the two roots of the equation

$$\nu=\{\tfrac{1}{2}(\nu_1^2+\nu_2^2)\pm\tfrac{1}{2}[(\nu_1^2-\nu_2^2)+4\mu^4]\}^{\frac{1}{2}}. \tag{2}$$

These two modes of oscillation are called the *normal modes*. Their importance is due to the fact that the general motion can always be represented as a combination of the normal modes. When expressed in this way, the solutions of the equations of motion are

$$\begin{aligned} x_1 &= \frac{A_+\cos\alpha}{m^{\frac{1}{2}}}\cos(2\pi\nu_+t-\Phi_+) \\ &\qquad +\frac{A_-\sin\alpha}{m^{\frac{1}{2}}}\cos(2\pi\nu_-t-\Phi_-), \\ x_2 &= \frac{A_+\sin\alpha}{m^{\frac{1}{2}}}\cos(2\pi\nu_+t-\Phi_+) \\ &\qquad -\frac{A_-\cos\alpha}{m^{\frac{1}{2}}}\cos(2\pi\nu_-t-\Phi_-). \end{aligned} \tag{3}$$

The four constants $A_+$, $A_-$, $\Phi_+$ and $\Phi_-$ are arbitrary, and their values are fixed by specifying initial displacements and velocities of the two masses; $\alpha$ is defined by the relations

$$\tan\alpha=\frac{\nu_1^2-\nu_+^2}{\mu^2}=\frac{\mu^2}{\nu_2^2-\nu_+^2}=\frac{-\mu^2}{\nu_1^2-\nu_-^2}=-\frac{\nu_2^2-\nu_-^2}{\mu^2}. \tag{4}$$

[1] P. M. Morse, *Vibration and sound* (McGraw-Hill, 1936), pp. 30–45.

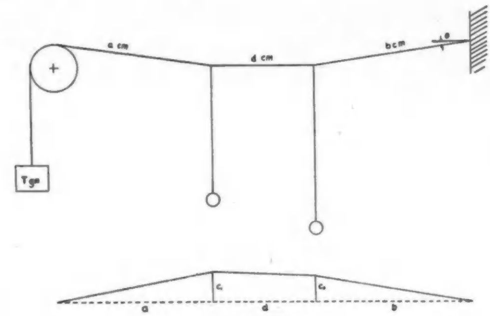


FIG. 1. Diagram of apparatus.

Two subdivisions can be introduced: (i) the case of resonance ($\nu_1=\nu_2$), and (ii) the case of distuning ($\nu_1\neq\nu_2$).

## Resonance

The experiments dealing with resonance will be considered first. Equation (2) becomes

$$\nu=(\nu_1^2\pm\mu^2)^{\frac{1}{2}},$$

and, because $\mu$ is much smaller than $\nu_1$ for the coupling encountered in this experiment, we can write,

$$\begin{aligned}\nu_+&=\nu_1+(\mu^2/2\nu_1),\\ \nu_-&=\nu_1-(\mu^2/2\nu_1),\\ \nu_+-\nu_-&=\mu^2/\nu_1.\end{aligned}\tag{5}$$

For the case we are considering, the two simple pendulums will oscillate with frequency $\nu_+$ if their initial displacements are equal in magnitude but opposite in direction. To produce the other normal mode of frequency, $\nu_-$, the initial displacements should be equal in magnitude and in the same direction. If these frequencies are measured experimentally as a function of force in the supporting string for constant distance of separation, or *vice versa*, $\mu^2$, and thus $K_3$, can be obtained as a function of these variables. A more accurate method of obtaining $K_3$ will be given presently, and this experiment may be performed primarily to direct attention to the fact that the two frequencies of the normal modes are always higher or lower than either of the natural frequencies of the pendulums and to give an indication as to the order of magnitude of $K_3$.

When a driver forces an oscillator at its natural frequency, theory predicts that displacement should lag behind the force by $\pi/2$ rad. This prediction can easily be verified by starting the pendulums in motion with the initial displacements $x_1=x_0$ and $x_2=0$ and the initial velocities equal to zero. For the first few seconds pendulum *1* is the driver, and the displacement of pendulum *2* is seen to lag by the expected $\pi/2$ rad.

Further study of the system put into motion under these initial conditions is very fruitful. The motion is not simple harmonic under these conditions but is a combination of the two normal modes and, as such, the displacements are given by Eqs. (3). These equations can be greatly simplified by applying the initial conditions and the resonance condition in addition to assuming small coupling. The resulting equations are

$$\begin{aligned}x_1&=\tfrac{1}{2}x_0(\cos 2\pi\nu_+t+\cos 2\pi\nu_-t),\\ x_2&=\tfrac{1}{2}x_0(\cos 2\pi\nu_-t-\cos 2\pi\nu_+t).\end{aligned}\tag{6}$$

On substituting the equivalents of $\nu_+$ and $\nu_-$ in terms of the resonance frequency $\nu_1$, then expanding and simplifying, we have, finally,

$$\begin{aligned}x_1&=x_0\cos(\pi\mu^2t/\nu_1)\cos 2\pi\nu_1t,\\ x_2&=x_0\sin(\pi\mu^2t/\nu_1)\sin 2\pi\nu_1t.\end{aligned}\tag{7}$$

It is thus seen that each oscillator has a vibration frequency $\nu_1$ with an amplitude oscillation of frequency $f$, of magnitude $\mu^2/2\nu_1$, secured by equating $2\pi ft$ and $\pi\mu^2t/\nu_1$. The period of energy exchange, $P$, is the reciprocal of $f$ and, as this is the quantity measured, we have $\mu^2=2\nu_1/P$. This leads directly to the coupling coefficient $K_3$.

*Effect of tensile force on coupling.*—To study the effect of tensile force on coupling it is convenient to use two pendulums of equal length, placed symmetrically on the supporting string ($a=b$ in Fig. 1, and $\nu_1=\nu_2$). Small lead spheres of mass about 100 gm serve as pendulum bobs. As long as the force applied to the end of the string passing over the pulley is quite large compared to the weight of the bobs, the angle $\theta$ between the string and a horizontal line at the fixed points (pulley and wall) will be quite small. Consideration of the three forces acting at the point of attachment of either pendulum to the supporting string leads directly to the conclusion that the product of $\tan\theta$ and the force $T$ in the portion of the string between the pendulums is constant; or, if $\theta$ is small, $T\theta$ is constant. Now, the true length of the pendulums is $l+a\sin\theta$, or approximately $l+a\theta$. The lateral displacement of the point of attachment of pendulum to supporting string for either of the pendulums will thus be directly proportional to $\theta$, and the coupling coefficient will therefore be proportional to $\theta$. Combining this result with the foregoing relation for $T$ and $\theta$, we have $TK_3=C'$, a constant. It should be observed that,

when $\theta$ is small, $T$ is approximately equal to the force applied to the end of the string passing over the pulley.

Experimentally, the period of energy exchange, $P$, is determined as a function of the tensile force in the supporting member. Using a number of different tensile forces and securing $K_3$ for each from the observed periods of energy exchange by means of the formula $K_3=8\pi^2\nu_1 m/P$, one finds the dependence of $K_3$ on tensile force (Fig. 2). The data displayed in Fig. 2 were secured by a student. It is to be noted that this curve is in close agreement with the theoretically predicted relationship, $TK_3=C'$.

*Effect of natural period on coupling.*—Since $\mu^2=2\nu_1/P$ and $\nu_1=(1/2\pi)(g/l)^{\frac{1}{2}}$, we have $\mu^2=(1/\pi P)(g/l)^{\frac{1}{2}}$. Also, $\mu^2=k_3/4\pi^2 m$, which leads to $k_3=(4\pi m/P)(g/l)^{\frac{1}{2}}$. If the pendulums are kept in resonance and a series of observations is made on period of energy exchange as a function of the common lengths of the two pendulums, it is possible to verify the fact that the product of $K_3$ and $Pl^{\frac{1}{2}}$ is a constant. A typical set of data secured by a student is displayed in Fig. 3.

*Effect of separation of the two pendulums on the coupling.*—The closeness of coupling of the two resonant pendulums depends on their distance of separation. The manner of variation may be investigated by keeping the force in the supporting string constant, choosing a convenient length for the pendulums and measuring the period of energy exchange as a function of separation. The coupling coefficient $K_3$ is calculated from the period of energy exchange as in the previous experiments.

As the percentage separation $100d/(2a+d)$ is increased, there is a reduction in the amount of

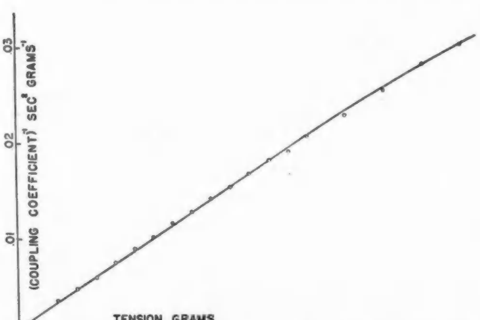


FIG. 2. Reciprocal of coupling coefficient $K_3$ *versus* tensile force.

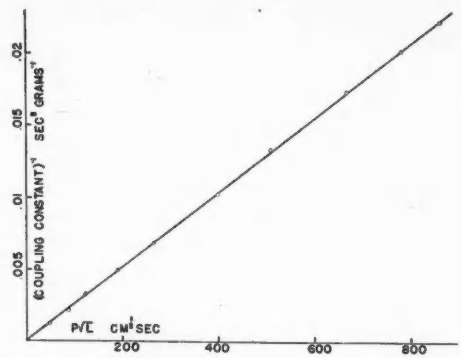


FIG. 3. Reciprocal of coupling coefficient $K_3$ *versus* $Pl^{\frac{1}{2}}$.

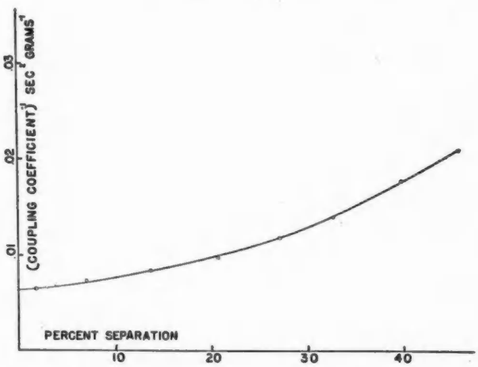


FIG. 4. Reciprocal of coupling coefficient $K_3$ *versus* percentage of separation.

lateral displacement of the point of attachment of either pendulum. The rate of decrease of this lateral displacement is less than the rate of increase of percentage separation. The coupling coefficient thus decreases slowly with increased separation of the pendulums. Superimposed on this is a practically linear decrease in coupling due to increased separation. (A given lateral motion of the point of attachment of one pendulum produces less and less coupling force on the second pendulum as their distance of separation increases.) The combination of these two effects accounts for the upward concavity of the curve of Fig. 4, which is a graph of the reciprocal of $K_3$ *versus* percentage separation. These results were also secured by a student.

## Distuning

When the natural frequencies of the two pendulums are not equal, the amplitude of the

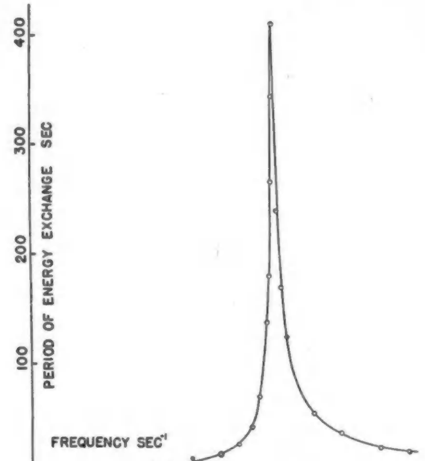


FIG. 5. Period of energy exchange *versus* frequency of the variable frequency pendulum.

pendulum which is originally displaced in order to start the system in motion will remain fairly constant, and will always be much larger than that of the second pendulum. While it cannot be said that this initially displaced pendulum exerts a simple harmonic force of constant amplitude on the other pendulum, many of the experimental results which are observed under such conditions are in approximate agreement with calculations obtained by considering the initially displaced pendulum to be a driver, and the other a driven oscillator. With the constant-length pendulum as the driver, the amplitude of the variable-length pendulum will be small, provided coupling is small and provided the variable-length pendulum is not in resonance.

If the pendulums are started in motion in a normal mode, then the amplitude ratio between them will be maintained and the steady-state amplitude of the driven oscillator can be calculated from the theory of forced oscillators.

If the distuned system is started according to the initial conditions,

$$x_1=x_0,\quad x_2=0,\quad (dx_1/dt)_0=(dx_2/dt)_0=0,$$

the motion will be nonperiodic, but rather simple solutions for $x_1$ and $x_2$ can be obtained from the general solutions of Eq. (3):

$$\begin{aligned} x_1&=x_0(\cos^2\alpha\cos 2\pi\nu_+t+\sin^2\alpha\cos 2\pi\nu_-t),\\ x_2&=x_0\sin\alpha\cos\alpha(\cos 2\pi\nu_+t-\cos 2\pi\nu_-t). \end{aligned}\tag{8}$$

Since $\tan\alpha[=-\mu^2/(\nu_1^2-\nu_-^2)]$ is small for small coupling, we see that $\cos\alpha$ is approximately unity and $\sin\alpha$ is approximately $-\mu^2/(\nu_1^2-\nu_2^2)$, $\nu_+$ being just a little larger than $\nu_1$, and $\nu_-$ just a little smaller than $\nu_2$. Therefore,

$$\begin{aligned} x_1&\cong x_0\left[\cos 2\pi\nu_+t+\frac{\mu^4}{(\nu_1^2-\nu_2^2)^2}\cos 2\pi\nu_-t\right],\\ x_2&\cong\frac{2\mu^2x_0}{\nu_1^2-\nu_2^2}[\sin 2\pi\tfrac{1}{2}(\nu_++\nu_-)t]\\ &\qquad\times[\sin 2\pi\tfrac{1}{2}(\nu_+-\nu_-)t]. \end{aligned}\tag{9}$$

It is apparent from examination of these equations that the amplitude of pendulum *1* will undergo small variations while pendulum *2* will have a small amplitude which is modulated with a frequency $\frac{1}{2}(\nu_1-\nu_2)$.

As the length of the variable-frequency pendulum approaches that of the driver, its amplitude of motion increases and the period of energy exchange increases. If the period of energy exchange is plotted as a function of the frequency of the variable pendulum, a typical resonance curve results. If the dissipative forces are small, the resonance curve is sharp. Typical results secured by students are shown in Fig. 5. Similar curves may be secured by plotting amplitude of response as a function of the variable frequency.

The amplitude of the variable-frequency pendulum is, according to Eqs. (9), $A=2\mu^2x_0/(\nu_1^2-\nu_2^2)$. Within experimental error, measurements of $A$ at various frequencies lead to a constant value for $\mu$ when substituted in this equation.

* * *

This experiment has been used during the past two years in connection with a laboratory course in mechanics for junior students majoring in physics. The results secured by all the students who have performed the experiment have been uniformly good, and in all cases the experiment has stimulated a great deal of interest in the physics of coupled oscillators.

The author wishes to thank Messrs. Voelker, Glaser and Dutton, undergraduate students at Case School of Applied Science, for securing the data used to illustrate this article.

# On the Accuracy of the Wheatstone Bridge

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1. *The problem.*—What is the setting of the slide-wire Wheatstone bridge for best accuracy, that is, for the least relative error in the measured resistance? The answer to this question depends on what elements of the set-up are subject to choice. The present discussion assumes that the only controllable elements are the standard resistance $S$ and the position of the sliding contact. In analyzing the error $\Delta X$ in the unknown resistance $X$, two sources of error are taken into account:

(*a*) The "leeway" in setting for balance, that is, the change in the position of the sliding contact producing a just noticeable value $i$ of the galvanometer current; the resulting error in the resistance $R_x$ of the portion of the slide wire corresponding to $X$ will be called $\Delta_1 R_x$.

(*b*) The "reading" error $r$ in the identification of the resistance of a portion of the slide-wire; if the latter is assumed to be uniform, this error is proportional to the error in reading the abscissa of the contact.

The author has been unable to find a textbook that discusses the combined effect of these two errors. The relative error in the standard resistance $S$ may be left out of consideration, since it is a constant to be simply added to the total error discussed here.

2. *The formula for the error.*—Let $E$ be the emf; $I$, the galvanometer current; $G$, $R_x$, $R_s$, the resistances of the galvanometer and of the two sections of the slide-wire, respectively; then $R_x+R_s=R$, a constant. The resistance of the battery branch is neglected. We have the familiar formula,

$$I=E\frac{XR_s-SR_x}{(X+S)(GR+R_xR_s)+XSR}.$$

To find the leeway error $\Delta_1 R_x$ at the balance position, characterized by the vanishing of the numerator, we note that $\Delta(u/v)=\Delta u/v$ if $u=0$, and that $\Delta_1 R_s=-\Delta_1 R_x$, since $\Delta_1 R=0$. Then we obtain, by differentiation,

$$\frac{\Delta_1 I}{\Delta_1 R_x}\equiv\frac{i}{\Delta_1 R_x}=\frac{-E(X+S)}{(X+S)(GR+R_xR_s)+XSR}.$$

From the condition that $XR_s=SR_x$ it follows that $XS/(X+S)=XR_s/R$. This permits us to transform the last equation into

$$|\Delta_1 R_x|=\frac{i}{E}(GR_x+GR_s+R_xR_s+XR)$$
$$=\frac{i}{E}\frac{R_xR_s}{R}\left(\frac{G}{x}+\frac{G}{s}+\frac{X}{x}+R\right), \quad (1)$$

where $x\equiv R_x/R$ and $s\equiv R_s/R$, so that $x+s=1$.

From the equation $X=SR_x/R_s$ we get, by logarithmic differentiation,

$$\frac{\Delta X}{X}=\frac{\Delta R_x}{R_x}-\frac{\Delta R_s}{R_s}=\left(\frac{1}{R_x}+\frac{1}{R_s}\right)\Delta R_x. \quad (2)$$

But $\Delta R_x=r+|\Delta_1 R_x|$, and, substituting the value for $|\Delta_1 R_x|$ from Eq. (1), we get from Eq. (2),

$$\frac{\Delta X}{X}=\left(\frac{1}{R_x}+\frac{1}{R_s}\right)r+\frac{R}{R_xR_s}|\Delta_1 R_x|$$
$$=\left(\frac{1}{R_x}+\frac{1}{R_s}\right)r+\frac{i}{E}\left(\frac{G}{x}+\frac{G}{s}+\frac{X}{x}+R\right).$$

In the expression after the last equality sign, factor out $i/E$, and put

$$H\equiv(E/i)(r/R) \quad (3)$$

to obtain

$$\frac{\Delta X}{X}=\frac{i}{E}\left(\frac{G+H+X}{x}+\frac{G+H}{s}+R\right). \quad (4)$$

This is a first expression for the relative error. The quantity $H$ has the dimensions of a resistance. For a Weston Model 375 galvanometer, for example, $R=10$ ohms, $r=0.01$ ohm, $G=20$ ohms, $i=10^{-6}$ amp; if $E=1.5$ v, $H=1500$ ohm.

3. *The error in terms of the minimum error.*—Factor out $G+H$ in the first two summands of the right-hand member of Eq. (4), and put $\xi\equiv X/(G+H)$ to obtain

$$\frac{\Delta X}{X}=\left(\frac{i}{E/G}+\frac{r}{R}\right)\left(\frac{1+\xi}{1-s}+\frac{1}{s}\right)+\frac{i}{E/R}. \quad (5)$$

The function of $s$ in parentheses has a minimum at $s_0$ defined by

$$s_0=1/[1+(1+\xi)^{\frac{1}{2}}]=$$
$$1/\{1+[1+X/(G+H)]^{\frac{1}{2}}\}. \quad (6)$$

Note

$$X_0/s_0=[1+X/(G+H)]^{\frac{1}{2}}.$$

The equality

$$\frac{1+\xi}{1-s}+\frac{1}{s}=\frac{1}{s_0^2}\left[1+\frac{(s-s_0)^2}{s(1-s)}\right]$$

is easily checked, after some simple algebra. The minimum value of the right-hand member of the last equation is $s_0^{-2}$, and thus the minimum of $\Delta X/X$ is

$$\left(\frac{\Delta X}{X}\right)_0=\frac{i}{E/R}+\left(\frac{i}{E/G}+\frac{r}{R}\right)\frac{1}{s_0^2}, \qquad (7)$$

and consists of a part $iR/E$, which is independent of the measured resistance, and a part that is a function of $X$, namely,

$$e(X)\equiv\left(\frac{i}{E/G}+\frac{r}{R}\right)\frac{1}{s_0^2}. \qquad (8)$$

The error formula, Eq. (5), can now be written

$$\frac{\Delta X}{X}=\frac{i}{E/R}+e\left[1+\frac{(s-s_0)^2}{s(1-s)}\right]. \qquad (9)$$

*4. The best measurement of a given resistance.*—(*a*) *The minimum error*. The error, for a given $X$, is a minimum when $s=s_0=[1+(1+X/(G+H))^{\frac{1}{2}}]^{-1}$. As $X$ increases from 0 to $\infty$, $s_0$ decreases from 0.5 to 0, that is, the best position of the sliding contact moves from the center of the wire (assumed to be uniform) to its end on the side of the standard resistance.

(*b*) *Latitude.* As $s$ varies from 0 to $\infty$, the error, given by Eq. (9), decreases from $\infty$ to the value given by Eq. (7), and then increases again towards $\infty$. Let us ask what latitude may be allowed for the position of the sliding contact. A fair measure of this latitude is the range over which the error does not exceed the minimum error by more than the value $e$ of Eq. (8). In fact, in this case the error is not quite twice the minimum error. From Eq. (9) we see that such an increase occurs when $(s-s_0)^2=s(1-s)$, or

$$2s^2-(2s_0+1)s+s_0^2=0.$$

The latitude is the difference of the two roots of this equation:

$$\text{Latitude }(X)=\tfrac{1}{2}(1+4s_0-4s_0^2)^{\frac{1}{2}}. \qquad (10)$$

This function of $s_0$ reaches its maximum $(1/2)^{\frac{1}{2}}=0.7$ at $s_0=0.5$, and falls off from there to the value 0.5 at $s_0=0$. For a uniform slide-wire of 100 cm length, this means a range of at least 50 cm, no matter what the set-up and the measured resistance. Hence the working rule for the freshman laboratory: *For a uniform slide wire, with the unknown resistance on the left of the 100-cm scale, balance the bridge between the 50-cm and 75-cm marks, to assure an error not exceeding twice the minimum error. For large resistances favor the 75-cm mark.*

The meaning of the word "large" is discussed in the next section. In the meantime, the following remark is, perhaps, not out of place, even if not related to our error discussion. The equation for the bridge current shows that $S=0$, $R_s=0$ (or $s=0$) gives a balance for every $X$. This can be used to dispense with a preliminary protective resistance, and to avoid unsystematic hunting for the balance point. Start with $S=0$, and $s=0$; there will be no galvanometer deflection. Then increase $S$ gradually and bring the galvanometer back to zero by increasing $s$, whenever a deflection is incipient. This is continued until the contact reaches the optimum region. The same procedure can be applied to the dial decade box to eliminate hit-and-miss fumbling.

*5. The best measurement with a given bridge; sensitivity.*—The minimum error for a given $X$, expressed by Eq. (7), is itself a minimum for $s_0=\frac{1}{2}$—that is, for $X=0$—and increases towards $\infty$ as $s_0$ tends towards zero. This means that the bridge favors small resistances, and fails altogether with large resistances. For the example already given, the error $\epsilon$ is computed from Eq. (7) as roughly $0.001/s_0^2$. If $\epsilon$ is of the order $10^{-2}$, this indicates a value for $s_0^2$ equal roughly to $(1000\epsilon)^{-1}$. An error of 1 percent is thus reached when $\xi=100$ or $X=150{,}000$ ohm. A resistance of this order could be described as "large," as far as the freshman laboratory is concerned; $s_0$ is then 0.32.

The least possible error, attained for $X=0$, is

$$\epsilon_0=iR/E+4(iG/E+r/R),$$

and can be used as a measure of the *sensitivity* of the set-up. This least error decreases with $E^{-1}$, $i$ and $r$, which is obvious, but reaches a minimum given by $(\epsilon_0)_{\min}=4(ri/E)^{\frac{1}{2}}$ at $R=2(rE/i)^{\frac{1}{2}}$. With the data already used, this optimum value of $R$ is 245 ohm (as against the conventional 10 ohm). This larger value has the additional advantage of smaller heat development.

The foregoing discussion holds only if the temperature (at the balance point, at least) is maintained constant. Otherwise it will have to be modified by the condition that $S$ cannot drop below a certain bound. In fact, *at the balance point*, the current in $X$ is $E/X+S$, and the power developed in $X$ is $E^2X/(X+S)^2$. With given surroundings, this power should not exceed the limit compatible with thermal equilibrium, which puts a lower bound under $S$.

# Equipment for Elementary Laue X-Ray Studies

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Many laboratories, especially in smaller colleges, experience difficulty in obtaining certain x-ray materials and equipment. It is for this reason that some experiments are described from which representative Laue patterns may be obtained despite the fact that the apparatus, apart from the x-ray tube and transformer, consists of "scraps" available in almost any laboratory. A few things will also be pointed out that otherwise would have to be learned firsthand, at the expense of valuable time and film.

The main problem, of course, is the x-ray unit. In lieu of a new tube, a secondhand tube may often be procured from a physician who has x-ray equipment. It should be a Coolidge type tube, and its operation will require a high-voltage transformer capable of supplying from 20,000 to 100,000 v. A milliammeter, carefully insulated from ground, should be connected in the anode lead. The tube should be tested for a good vacuum in the usual manner by the use of a high-frequency spark coil. This very simple arrangement requires that the tube be used as its own rectifier, and is entirely adequate for many purposes. A rectifying unit entails considerably more labor and equipment than might be available to the average small laboratory.

The only other item of importance is adequate shielding for the equipment. The operator should protect himself from the x-rays by the use of special lead glass and lead sheeting. Overexposure to x-rays will cause severe burns and temporary or permanent sterility. The operator should carry in his pocket a piece of x-ray film wrapped in black paper and fastened with a paper clip. A small dental x-ray film is suitable. In this case, one should be sure to place the stiff back of the envelope, which may contain lead foil, next to the body. If after a week of work the film shows an image of the clip upon development, the intensity of x-rays has been too high.

Table I gives the minimum thicknesses of lead which are necessary when the tube is operating at the indicated voltages.[1] The lead glass envelope which encases the tubes of most small medical units generally provides adequate shielding over the enclosed area.

Most Laue photographs are made by using two pinholes. Such pinholes can be made with a fine needle or a fine wire drill (about No. 64 for good detail). A simple combination of the two pinholes is illustrated in Figs. 1 and 2. A tin can is soldered

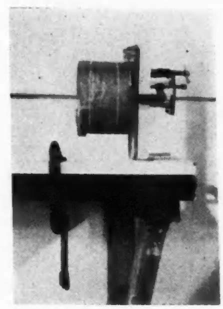

Fig. 1. Arrangement of lead shielding in the pinhole system.

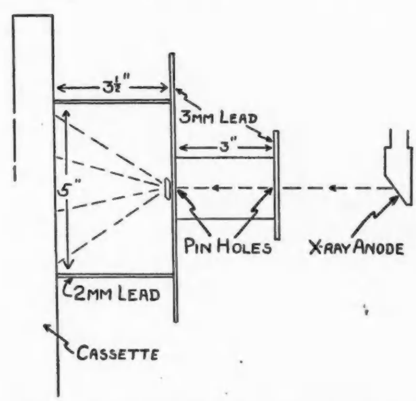


Fig. 2. Sketch of pinhole camera and cassette for Laue patterns.

Table I. Minimum thickness of lead for shielding.

| Voltage (kv) | Thickness (mm) |
|---|---|
| 75 | 1.0 |
| 100 | 1.5 |
| 150 | 2.5 |
| 200 | 4.0 |

* Present Address, Mine Safety Appliances Company, Pittsburgh 13, Pennsylvania.

[1] G. L. Clark, *Applied x-rays* (McGraw-Hill, ed. 2).

FIG. 3. Laue pattern for a potassium dichromate crystal 4.5 mm thick, using one pinhole; 30-min exposure at from 0.5 to 1.0 ma.

between the two lead sheets for rigidity. The problem of alinement of the pinholes with the "hot spot" of the anode is one that requires care. A fluorescent screen or chemical may be used to determine when the x-rays are passing directly through the two pinholes. Alinement may also be accomplished after some practice by sighting through the two holes. It is advisable, after alinement, to fix the tube and the pinholes permanently.

For careful work, two pinholes should be used. This gives patterns with smaller spots, as usually seen in published articles. However, much interesting work can be done with only a single pinhole. The central spot will consist of an image of the anode, while the deviated beams will form very good though larger spots (see Fig. 3). The single pinhole method has the advantage that alinement is far easier and the exposure time is shorter—30 min, as opposed to 2 hr. In the present work the crystals were supported directly over the exit pinhole with some soft wax. Obviously, much more symmetrical patterns could be obtained if the crystals were oriented with more care. An inexpensive ruling pen makes a good holder for crystals. The handle is very easily clamped into position, and the adjustable tip can be made to hold several sizes of crystals.

Difficulty is experienced in converting a medical unit to use with long exposures. For continuous use the anode current must be cut to a fraction of its rating for common medical use. However, the tube is still likely to become overheated. This can be relieved to some extent by playing an electric fan onto the radiator at the anode end of the tube. Even so, it is wise to stop the tube every few minutes and, after making sure the equipment is shut off, to check for overheating by feeling the radiator. If the transformer is in a cabinet, difficulty may also be encountered when the high potential, after a few minutes, causes the confined air to break down and allow "arcing over." The equipment should be carefully protected by fuses against this possible overload. The arcing over can be reduced considerably by having another electric fan circulate air into the cabinet.

A lead-backed film holder should protect the rear of the film sufficiently from secondary radiation. If homemade holders are used, appropriate lead backing should be included in the set-up.[2]

Acknowledgment is due Dr. W. L. Hole for his advice and Dr. G. O. Sharp for numerous large crystals from the inorganic preparations class.

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[2] Many articles in the *American Journal of Physics* deal with phases of this problem that will be useful to anyone entering the field for the first time. Similar set-ups are described by Wadlund **6**, 103 (1938), and Weber, McGee and Gerhard **5**, 279 (1937). Voltage measurement with a cathode-ray oscillograph is discussed by Edwards **8**, 253 (1940). Kirkpatrick **8**, 319 (1940), offers a model for demonstrating Laue patterns. An intermittent control device for the x-ray tube is described by Weber and Grill **9**, 381 (1941). Two books that will be found useful are: Harnwell and Livingood, *Experimental atomic physics* and Wyckoff, *The structure of crystals*.

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*I BELIEVE that the time given to refutation in philosophy is usually time lost. Of the many attacks directed by many thinkers against each other, what now remains? Nothing, or assuredly very little. That which counts and endures is the modicum of positive truth which each contributes. The true statement is, of itself, able to displace the erroneous idea, and becomes, without our having taken the trouble of refuting anyone, the best of refutations.*—HENRI BERGSON.

# Demonstrating Linear Thermal Expansion by Using the Catenary

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AND

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THE purpose of this experiment is to demonstrate the elementary facts of linear expansion to a large group. Several horizontal bare wires of uniform cross section, to which an electric potential difference is applied, are supported by two vertical posts. The wires sag into a catenary when they are heated. Paper riders show the positions of the cold wires, these riders burning off when the wires become red hot, at which time no auxiliary indicator is needed.

A minimum of apparatus is required, and, as shown in Fig. 1, no concealed equipment is employed. There are no wheels, axles, mirror attachments or optical levers; just hot wires in clear view. In the figure only one of the expanding wires is shown.

Electric linemen know that to reel out an extra foot of wire on a long span will cause a large sag in the wire. You might ask yourself this question before you figure it out accurately: If a wire 1001 ft long is strung between two posts 1000 ft apart, what would be the sag in the middle? Would you be surprised to know that it is 20 ft? Actually, an increase in arc of 1 ft in 1000 (0.1 percent) causes a sag of 20 ft, or 2 percent of the chord. In employing this effect to demonstrate linear expansion, it is convenient to use a chord of 100 cm, in which case an increase of 0.1 percent in length causes a sag of 2 cm. Data on the wires used in this experiment are given in Table I.

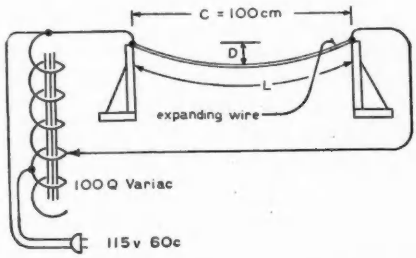


FIG. 1. Schematic drawing of the expansion apparatus.

To reduce the initial strain, an initial sag of about 1.0 cm is desirable for the wires of small tensile strength. In operation, the voltage is gradually increased by means of a Type 100Q Variac (primary 115 v, 60 c/sec) whose maximum rated output current is 18 amp. When the 10-amp fuse wire, the copper wire and the iron wire are connected in parallel, they will burn out in the order named as the potential difference is increased. The fuse wire melts without becoming red hot, the copper wire at red heat, and the iron wire at a somewhat higher temperature.[1] A few seconds suffice to complete the demonstration, so that stretching caused by softening at increased temperature is minimized.

It may be of interest to mention that the fuse wire used in this experiment, when it burns out, falls in several

TABLE I. Data on wires used.

| Wire | Diameter (in.) | Approximate values to melt 100 cm of wire in air $I$ (amp) | $V$ (v) |
|---|---|---|---|
| *Pure metals* | | | |
| Tin | 0.031 | 14 | 9 |
| Lead | .032 | 12 | 14 |
| Aluminum | .0253 | 25 | 16 |
| Silver | .020 | 25 | 14 |
| Copper | .0159 | 20 | 23 |
| Nickel | .0246 | 24 | 70 |
| Iron | .018 | 10 | 67 |
| Palladium | .020 | 16 | 50 |
| Platinum | .020 | 18 | 57 |
| Molybdenum | .025 | 27 | 78 |
| Tantalum | .025 | 16 | 80 |
| Tungsten | .025 | 10 | 190 |
| *Alloys* | | | |
| Fuse | .036 | 12 | 11 |
| Brass | .0179 | 18 | 21 |
| Nichrome V | .0216 | 14 | 60 |

* Lieutenant USNR, on leave from State Teachers College, Indiana, Pennsylvania. The assertions herein are the private ones of the writers, and are not to be construed as official or reflecting the views of the Navy Department or of the Naval Service at large.

[1] Iron wire permits the demonstration of recalescence; see Sutton (ed.), *Demonstration experiments in physics* (McGraw-Hill, 1938), p. 197.

segments, each about 1 cm in length. Tin and aluminum behave the same way. These wires apparently become brittle when near the melting point; after cooling the broken pieces are as ductile and as soft in temper as the original wires. Catching the wires on a piece of cardboard placed about 1 in. below the sagging wire prevents the segmented breaking, and the wire stays in several long pieces. However, if the wire sags, then melts and drops about 20 cm to the table, dozens of segments form.

Of the metals tested, the three with the highest melting points are molybdenum, tantalum and tungsten. Applying the procedure outlined previously, one finds that the action of the molybdenum and tungsten is more fireworks than physics. Molybdenum gives a particularly spectacular demonstration, oxidizing rapidly at white heat, giving off white filaments that float away in waves. When the wire finally burns out, white hot segments break off; suitable precautions must be taken. Hot tungsten[2] also oxidizes in air, as can be conveniently shown with the aid of a lamp bulb in which air has replaced the usual inert gas. The oxides of the hot molybdenum and tungsten do not adhere to the parent wires; these wires become thinner during the experiment.

Red hot tantalum quickly acquires a hard white oxide coating, several times the diameter of the parent wire, which may be strong enough (i) to support the central wire beyond the proper breaking point, or (ii) to hold it in place, thus preventing normal lengthening. Either of these conditions gives a false idea of the amount of thermal expansion. The use of a transparent tube filled with an inert gas would be an answer to these oxidation troubles, but for the present purpose was not thought to be justified.

Wires that burn out at red heat, or at still higher temperatures, may afford a visual example of "hot spots" in electrical machinery. If the wire is slightly undersize for a fraction of its length,[3] this fraction becomes progressively hotter than adjacent portions when a current is set up in the wire. The results are cumulative with wires having positive temperature coefficients of resistance; the smaller size rises to a higher initial temperature, has more resistance per unit length

[2] Note from Table I that to melt the 100-cm, 0.025-in. tungsten wire requires 190 v; this cannot be obtained with the Variac of Fig. 1. If the length of the 0.025-in. tungsten wire is made 50 cm, the Variac is suitable. For the test on the 100-cm tungsten wire reported in Table I, we used one phase of a three-phase 220-v generator and varied the field for control of the output voltage.

[3] Some wires have sections as long as 20 cm with a noticeably reduced area as compared with adjoining sections. In obtaining data for Table III, many specimens of wire had to be rejected because of nonuniform diameter.

TABLE II. Catenary characteristics for a chord length $C$ of 100 cm.

| Arc length, $L$ (cm) | $\Delta L/C$ (percent) | $D/C$ (percent) |
|---|---|---|
| 100.027 | 0.027 | 1.00 |
| 100.106 | .106 | 2.00 |
| 101.0 | 1.0 | 6.14 |
| 102.0 | 2.0 | 8.72 |
| 103.0 | 3.0 | 10.7 |
| 104.0 | 4.0 | 12.4 |
| 105.0 | 5.0 | 13.9 |

resulting in greater energy dissipation in that length, and thus a still higher temperature occurs. Finally, the wire fails at the "hot spot" while other portions may be relatively cool.

A rapid method for finding the approximate sag of a catenary is provided by the formula for the length of a parabola,[4] and is good to 0.5 percent for values of sag up to 6 percent of the chord.[5] Thus,

$$L=C\left[1+\frac{2}{3}\left(\frac{2D}{C}\right)^2\right],$$

where $L$ is the length of the catenary, $C$ is the length of the chord, which is constant, and $D$ is the sag in the center at the time when the catenary length is $L$. From this approximate formula, we get

$$L-C=\Delta L=8D^2/3C,$$

so that

$$\Delta L_1/\Delta L_2=(D_1/D_2)^2.$$

As an example, if two catenary lengths are 101 and 102 cm, each with a constant chord length of 100 cm, and if the sag for the 101-cm arc is 6.14 cm, then the sag for the 102-cm arc is $6.14\sqrt{2}$ cm, or 8.68 cm.

Table II gives values of interest in using the catenary for this experiment, these being found from tables[6] rather than from the approximate formula. Figure 2, a logarithmic graph of $\Delta L/C$ *versus* $D/C$, can be used to get intermediate values.

Experimental results are given in Table III, where it may be seen that the total percentage linear expansion from room temperature to the melting point is roughly the same for the metals tested, except for those designated by parentheses

[4] Hudson, *Engineers manual* (Wiley, 1939), ed. 2, p. 17.

[5] Knowlton (ed.), *Standard handbook for electrical engineers* (McGraw-Hill, 1941), p. 1236.

[6] Thomas, Trans. A. I. E. E. **30**, part III, 2238–9 (1911).

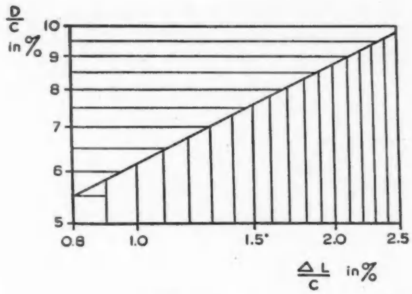


FIG. 2. Catenary characteristics.

in the first and last columns, despite the fact that the melting points vary widely.

It was first pointed out by Grueneisen[7] that, for monatomic elements, the percentage volume expansion from absolute zero to the melting point should be about the same and equal to 8 percent. This means that the maximum percentage *linear* expansion should be one-third of this value, about 2.7 percent. The next few paragraphs will show that, on the basis of Grueneisen's theory, the percentage linear expansion from room temperature to the melting point should be slightly more than 2 percent.

According to Grueneisen, the linear expansivity $\alpha$ of any monatomic solid varies with the temperature according to the relation

TABLE III. Experimental data on linear expansion.

| Wire | Melting point $T_m$ (°K) | $D$ at room temp. $T_r$, 300°K (cm) | $D$ at melting point $T_m$ (cm) | $\Delta L/L_r$ ($T_r$ to $T_m$) (percent) |
|---|---|---|---|---|
| *Pure metals* | | | | |
| (Tin) | 505 | 1.0 | 4.8 | (0.4) |
| (Lead) | 601 | 1.0 | 6.6 | (0.8) |
| Aluminum | 931 | 1.1 | 9.6 | 1.9 |
| Silver | 1234 | 0.2 | 9.4 | 2.2 |
| Copper | 1360 | 1.0 | 10.2 | 2.2 |
| Nickel | 1730 | 0.2 | 10.2 | 2.6 |
| Iron | 1800 | .2 | 9.3 | 2.2 |
| Palladium | 1830 | .2 | 9.7 | 2.4 |
| Platinum | 2050 | .2 | 9.1 | 2.1 |
| Molybdenum | 2900 | .2 | 8.6 | 1.8 |
| Tantalum | 3300 | .6 | 9.9 | 2.2 |
| Tungsten | 3660 | .2 | 9.5 | 2.3 |
| *Alloys* | | | | |
| (Fuse) | 450 | 1.0 | 6.5 | (0.8) |
| Brass | 1200 | 0.2 | 9.3 | 2.2 |
| (Nichrome V) | 1670 | .2 | 17.2 | (7.5) |

[7] E. Grueneisen, Ann. physique **30**, 296 (1912). The authors are indebted to H. Margenau for this reference.

$$\alpha=\frac{1}{3}\left(\frac{\beta}{c_v}\right)_0\frac{c_v}{[1-\delta(\beta/c_v)_0u]^2}, \tag{1}$$

where $\beta$ is the volume expansivity; $c_v$ is the molar heat capacity at constant volume; $(\beta/c_v)_0$ is the limiting value of the ratio $\beta/c_v$ as $T$ approaches 0°K; $u$, or $\int_0^T c_v dT$, is the molar energy; and $\delta=(m+n+3)/6$, where $m$ and $n$ are the exponents in the attraction and repulsion terms, respectively, of the equation relating the potential energy to the distance between vibrating atoms.

The essential features of Eq. (1) are exemplified by means of the graph shown in Fig. 3. At temperatures below the Debye characteristic temperature $\Theta$, the quantity in brackets is almost equal to unity and $\alpha$ varies as $c_v$. Above the Debye temperature, $c_v$ is almost constant at the Dulong and Petit value of $3R$, and the rise in the value of $\alpha$ is due almost entirely to the quantity in brackets. These features have been amply verified experimentally by Grueneisen[8] and by Nix and MacNair.[9] In the case of nickel, the Grueneisen relation holds quite well except in the region from 325° to 800°K, where the experimental curve lies slightly above the theoretical curve, reaching a small but sharp peak at the Curie point, 635°K. The disagreement between the regular Grueneisen curve and the experimental curve in the case of iron is more pronounced, there being two anomalous regions. From about 400° to 1000°K the experimental curve lies above the theoretical one and has a flat peak at the Curie point. At about 1100°K the linear expansivity becomes negative.

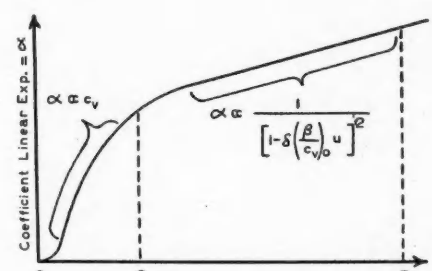


FIG. 3. The variation of linear expansivity $\alpha$ with temperature $T$, according to Grueneisen's relation.

[8] E. Grueneisen, *Handbuch der Physik* (1926), vol. X, p. 43.

[9] F. C. Nix and D. MacNair, Physical Rev. **60**, 597 (1941).

TABLE IV. Experimental data for metals.

| Metal | Melting point $T_m$ (°K) | Debye temp. $\Theta$ (°K) | $(\beta/c_v)_0$ $\left(10^{-6}\frac{\text{mole}}{\text{cal}}\right)$ | $\delta$ | $B=(\beta/c_v)_0\cdot T_m$ $\left(\frac{\text{mole °K}}{\text{cal}}\right)$ | $\frac{\Theta}{T_m}$ | $\frac{T_r}{T_m}$ |
|---|---|---|---|---|---|---|---|
| Sn | 505 | 185 | 9.0 | 6.3 | 0.00454 | 0.37 | 0.59 |
| Pb | 601 | 90 | 12.3 | 3.7 | 0.00740 | 0.15 | 0.50 |
| Zn | 693 | 320 | 13.5 | 4.0 | 0.00935 | 0.46 | 0.43 |
| Al | 931 | 419 | 12.0 | 2.7 | 0.0112 | 0.45 | 0.32 |
| Ag | 1234 | 229 | 8.9 | 3.3 | 0.0110 | 0.19 | 0.24 |
| Au | 1340 | 175 | 6.73 | 3.4 | 0.00903 | 0.13 | 0.22 |
| Cu | 1360 | 335 | 8.33 | 2.8 | 0.0113 | 0.25 | 0.22 |
| Ni | 1730 | 413 | 6.6 | 4.0 | 0.0114 | 0.24 | 0.17 |
| Fe | 1800 | 462 | 6.0 | 3.7 | 0.0108 | 0.26 | 0.17 |
| Pd | 1830 | 275 | 5.75 | 4.1 | 0.0106 | 0.15 | 0.16 |
| Pt | 2050 | 233 | 4.28 | 5.0 | 0.00878 | 0.11 | 0.15 |
| Mo | 2870 | 379 | 2.80 | 5.2 | 0.00804 | 0.13 | 0.10 |
| Ta | 3300 | 245 | 3.19 | 3.7 | 0.0105 | 0.074 | 0.09 |
| W | 3620 | 305 | 2.45 | 5.7 | 0.00886 | 0.084 | 0.08 |

Alloys are known to possess expansion curves with very high peaks, owing to order-disorder transitions.

An analysis of existing experimental data on the temperature variation of the linear expansivity of pure metals has yielded values of the constants $(\beta/c_v)_0$ and $\delta$ for 14 different metals which are listed in columns four and five of Table IV. Values of $\delta$ are seen to lie roughly between 3 and 6, without any apparent order. The values of $(\beta/c_v)_0$, however, show an interesting regularity when compared with the melting temperatures. Except for tin and lead, the values of $(\beta/c_v)_0$ are approximately inversely proportional to the melting temperature. The product $B\ [=(\beta/c_v)_0\cdot T_m]$ is listed in column six of Table IV and is seen to be in the neighborhood of 0.01 mole °K/cal for most of the metals.

In the temperature range from room temperature, $T_r=300$°K, to the melting point $T_m$, there is very little variation of $c_v$ since most of this range lies above the Debye temperature. Also, within this range, $u$ is a linear function of $T$. We may therefore write with very little error:

$$\left.\begin{aligned} (\beta/c_v)_0&=B/T_m,\\ c_v&=3R,\\ \int_0^T c_v\mathrm{d}T&=3RT-2\Theta, \end{aligned}\right\} T_r<T<T_m$$

whence the Grueneisen equation becomes

$$\alpha=\frac{1}{3}\frac{B}{T_m}\frac{3R}{[1-(B\delta/T_m)(3RT-2\Theta)]^2}. \tag{2}$$

Therefore, the fractional expansion from room temperature $T_r$ to the melting point $T_m$ is

$$\frac{\Delta L}{L_r}=\frac{BR}{T_m}\int_{T_r}^{T_m}\frac{\mathrm{d}T}{\left[1+\frac{2B\delta\Theta}{T_m}-\frac{3BR\delta}{T_m}\cdot T\right]^2}$$

$$=\frac{BR}{T_m}\cdot\frac{T_m}{3BR\delta}\left[\frac{1}{1+\frac{2B\delta\Theta}{T_m}-3BR\delta}-\frac{1}{1+\frac{2B\delta\Theta}{T_m}-3BR\delta\frac{T_r}{T_m}}\right].$$

When the products of small quantities are neglected, this reduces to

$$\frac{\Delta L}{L_r}=BR\frac{1-(T_r/T_m)}{1-3BR\delta\left(1+\frac{T_r}{T_m}-\frac{4\Theta}{3RT_m}\right)}. \tag{3}$$

An idea of the magnitude of the expansion of any typical pure metal may be obtained by substituting rough average values in Eq. (3). Thus, taking $B=0.01$ mole °K/cal, $R=2$ cal/mole °K, $T_r/T_m=0.2$, $\delta=4$, $\Theta/T_m=0.2$, we get

$$\frac{\Delta L}{L_r}=0.02\frac{0.8}{1-0.24(1+0.2-\frac{2}{3}\times0.2)}$$

$$=0.02\times0.8/(1-0.26)=2.2\text{ percent.}$$

TABLE V. Percentage values of the fractional expansion $\Delta L/L_r$ from room temperature to the melting point.

| Metal | Catenary method | Graphical integration | Calculated from Eq. (3) |
|---|---|---|---|
| Sn | 0.4 | 0.46 | 0.49 |
| Pb | .8 | .93 | .96 |
| Zn | | 1.35 | 1.4 |
| Al | 1.9 | 1.79 | 1.9 |
| Ag | 2.2 | 2.18 | 2.2 |
| Au | | 1.74 | 1.8 |
| Cu | 2.2 | 2.18 | 2.2 |
| Ni | 2.6 | 2.61 | 2.6 |
| Fe | 2.2 | 2.3 | 2.4 |
| Pd | 2.4 | 2.36 | 2.5 |
| Pt | 2.1 | 1.99 | 2.1 |
| Mo | 1.8 | 1.87 | 1.9 |
| Ta | 2.2 | 2.50 | 2.5 |
| W | 2.3 | 2.19 | 2.4 |

Substituting in Eq. (3) the individual values of the constants listed in Table IV, we calculated the respective values of the percentage expansion appropriate to each metal. In Table V these values are compared with the values of $\int_{T_r}^{T_m}\alpha dT$ obtained by measuring the areas under the experimental curves for $\alpha$ *versus* $T$. It is seen that the agreement is good even in the cases of nickel and iron, which do not follow the Grueneisen relation throughout the whole temperature range. In the case of iron, the region that lies above the Grueneisen curve probably cancels that which lies below.

The experimental values obtained by the catenary method agree fairly well with the predicted values, particularly in the case of metals that do not oxidize rapidly while expanding.

The main purpose of this paper, however, has been to describe an experiment suitable for demonstration to an elementary group rather than for precise calculation, the objectives being to show that:

(1) An increase in temperature causes an increase in length, the increase in temperature being shown for some of the wires by their visible radiation, but the increase in length for all being shown by increased sag.

(2) Upon heating from room to melting temperature, the maximum fractional expansions for several dissimilar metals are of the same order of magnitude, about 2 percent.

(3) Metals having high melting points have small linear expansivities, a corollary of the preceding conclusion.

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# American Physicists at War: from the First World War to 1942*

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*The true and lawful goal of the sciences is this: that human life be endowed with new discoveries and powers.*—Lord Bacon.

*Today, however, in the sight of the ferocious wolf preparing to set on our flock, and of our pastors united for the common defense, it does not seem to me any longer proper to hold these things [scientific discoveries of use in warfare] hid, and I have resolved to publish them partly in writing, and partly by word of mouth, for the benefit of Christians so that all should be in a better state either to attack the common enemy or to defend themselves against him.*—Nicolò Tartaglia.

## The First World War

AMERICAN physicists, like their brother scientists, served the country during the years of the first World War long before a declaration of war had actually been made on Germany. Their services were coordinated mainly through two agencies: the Naval Consulting Board and the National Research Council. The Council is of more interest for several reasons: first, its scope was much broader and its activities encompassed a much greater range of subject; second, it has outlasted its war service and has become a most important agency in the peacetime structure of American science.

Soon after the German attack on the *Sussex* increased the tension of our relations with Germany, the National Academy of Sciences, at a meeting in April 1916, unanimously passed a resolution to inform the President of the United States that "in the event of a break in diplomatic relations with any . . . country, the Academy desires to place itself at the disposal of the Government for any service within its scope."[1]

* The writer would like to acknowledge the kind assistance offered him in the preparation of this article by many persons: Professors Theodore Lyman and G. W. Pierce of Harvard University; Dr. Robert A. Millikan of the California Institute of Technology; Dr. Frank B. Jewett, President of the National Academy of Sciences; Dr. Paul Brockett, Executive Secretary of the National Academy of Sciences; Dr. F. R. Moulton, Permanent Secretary of the American Association for the Advancement of Science; Dr. Ross Harrison and the late Dr. Albert L. Barrows of the National Research Council; Mr. Archibald MacLeish, formerly Director of the Office of Facts and Figures. Lt. (j.g.) Bernard Barber, USNR, helped the writer to gather and organize the materials presented in this article. The writer, of course, assumes full and sole responsibility for all statements made. In the section dealing with the first World War, it must be borne in mind that the official records reveal the activities of committees and commissions and their members, but frequently fail to give the names of others who worked on research projects but who remain anonymous. A complete technical history of the achievements during the first World War should some day be written in full by an author who will be able to go beyond the official records and search through patents and other sources. This must, unfortunately, await the time when all such matters are removed from the classification of "restricted" or "confidential," and secrecy concerning them is no longer vital to our security.

[1] Report of the National Academy of Sciences (1916), pp. 12, 122.

President Wilson, upon receipt of the resolution, directed the Academy to take the initiative in finding out and coordinating the scientific resources of the Nation. Thereupon, the Academy organized the body which it called the National Research Council, an "independent body" with "power to act." The explicit purposes of its creation may be summarized as follows:

1. To prepare a national inventory of equipment for research, of the men engaged in it, and of the lines of investigation pursued in cooperating Government bureaus, educational institutions, research foundations and industrial research laboratories.
2. To act as a clearing house for the coordination of research in various departments of science.
3. To promote cooperation in research in order to secure increased efficiency, but at the same time avoiding any interference with individual freedom and initiative.
4. To support the efforts of educational institutions to secure larger funds and more favorable conditions for the pursuit of research and for the training of students in the methods and spirit of investigation.
5. To encourage researches designed to strengthen the national defense and to render the United States independent of foreign sources of supply liable to be affected by the war.[2]

An inspection of this program shows that only the fifth aim seems to be specially concerned with the immediate problems of defense. For the rest, the Council's activities were directed to improving the state of scientific research in the United States. With great wisdom, the planners of the National Research Council saw well that the technical aid which science would be able to render the nation in any future emergency (even one a few years ahead) would depend primarily upon the state of scientific research. The National Research Council saw as its job the organization of scientific endeavor by promoting cooperation and increasing research facilities; thus might science give to the nation its fullest measure of service. At the same time, the Council was building for the peace which was to follow war as well as for the war itself. In the words of Elihu Root,

> [Scientists cannot be] limited to a military objective, for when the war is over the international competitions of peace will be resumed, and the prizes of industrial leadership will fall to the nation which organizes its scientific forces most effectively.

The original planners of the National Research Council were: Edwin G. Conklin, distinguished biologist and now President of the American Philosophical Society; Simon Flexner, then director of the Rockefeller Institute; Robert A. Millikan, Nobel laureate in physics and the present head of the California Institute of Technology; Arthur A. Noyes, one of America's foremost chemists, famous for his work in the field of analysis, and Professor of Chemistry at the Massachusetts Institute of Technology; and George Ellery Hale, chairman.

George Ellery Hale (1868–1938)[3] was one of America's most distinguished astronomers and one of the world's leading men of science. Interested in the possibilities of large telescopes, he dreamed and planned the 200-in. reflector now under construction by the Carnegie Institution of Washington. Hale's activities were of an exceptionally broad nature for one who contributed so much to science. They involved literature and art, the history of early science, philosophy, archaeology and international relations. He was equally able to advise the late Henry E. Huntington on the best plan for the Huntington Library and to plan the activities of the National Research Council.

Hale was always active in the promotion of science, both national and international. He always remained an internationalist in spirit, even during the war years, and, as a scientist, he realized that science knows no boundaries of race or nationality, but only the boundary between mediocrity and greatness. In token of his international spirit, he served for many years as Foreign Secretary of the National Academy of Sciences and was a member of the International Council of Scientific Unions.

As able an administrator as he was a scientist, Hale realized that research in solar phenomena could be successful only if the work to be done were properly organized and distributed, and he was therefore a prime mover in the International Union for Cooperation in Solar Research. Though at heart he was always first and foremost an investigator, yet he expended a great deal of his time and energy as an organizer. He undertook such tasks as a necessary occupation only, never from primary desire. He recognized his own organizational ability and felt that it must be put to work in the service of science. In a letter to the great archaeologist, James H. Breasted, he wrote:

[2] Reference 1. See also "A history of the National Research Council," Reprint and Circular Series of the National Research Council, No. 106 (1933), pp. 7 ff.

[3] The biography of Hale is still to be written. An account of his life with a full bibliography of his writings may be found in National Academy of Sciences, Biographical Memoirs **21**, 181–241 (1940).

Secretary Josephus Daniels, Assistant Secretary Franklin Delano Roosevelt, the Naval Consulting Board and Navy Department representatives.

> The true investigator in science, if he be worthy of the name, must try to shape his life's work so as to secure the greatest possible accomplishment, even if it means a sacrifice of his own possibilities of personal research.[4]

The National Research Council, formed under Hale's aegis, benefited from his wisdom and his experience, and its planned organization constantly reflected his genius and his patriotism. Composed of the leading American scientific investigators and administrators, educators and engineers, it comprised the chiefs of the technical bureaus of the Army and the Navy, the heads of Government bureaus engaged in scientific research, representatives of the Smithsonian Institution, educational institutions, research endowments, research divisions of industrial and manufacturing establishments.[5] The active cooperation of the leading scientific and engineering societies was assured from the outset. The Engineering Foundation, for example, voted to apply its entire income for the year toward the expense of organization, to give the services of its Secretary, Dr. Cary T. Hutchinson, and to provide a New York office for the National Research Council. Formal organization took place at a meeting held in the Engineering Societies Building in New York City on September 20, 1916.[6] A few months after its inception, it became affiliated with the larger body that was coordinating all defense activity, the Council of National Defense. In the spring of 1917, the National Research Council was requested to act as the "Department of Science and Research" of the larger organization.

The entrance of the United States into the war was marked by the following message, sent to various scientific societies in allied countries:

> The entrance of the United States into the war unites our men of science with yours in a common

[4] Quoted in F. B. Seares, "George Ellery Hale (1868–1938)," Isis **30**, 241–267 (1940). On page 241, Seares lists a series of articles touching on various aspects of Hale's career. Seares also quotes extensively from unpublished "Biographical notes" left by Hale.

[5] A complete list of these institutions and organizations may be found in reference 1, p. 33.

[6] The organizing is fully described by C. T. Hutchinson, "Report on the origin, foundation, and scope of the National Research Council," Engineering Foundation, Publication No. 1 (Feb. 27, 1917).

> cause. . . . The National Academy of Sciences, acting through the National Research Council . . . would gladly cooperate in any scientific researches still underlying the solution of military or industrial problems.

One of the fruits of this was the famous inter-Allied scientific conference held in Washington in the spring of 1917, which largely was the concern of physicists. But before seeing the effect of that conference on our scientific research, let us look briefly at the other participating body, the Naval Consulting Board.

The official historian of the Naval Consulting Board tells us[7] that its creation "was a radical departure from the existing policies of the Navy Department." The Secretary of the Navy, Josephus Daniels, was anxious to implement the existing instrumentalities of war by new devices and inventions in order to render our navy the equal, if not the superior, of other navies. To that end he consulted with Thomas Alva Edison about the ways in which his idea could be realized in fact. At first called the "Naval Advisory Board," the Naval Consulting Board of the United States was formally organized under Edison's chairmanship on October 7, 1915. The members were chosen by having the presidents of 11 engineering and scientific societies select two delegates each. Edison and M. R. Hutchison were selected, in addition, by the Secretary of the Navy. Its full membership was as follows:

Lawrence Addicks, consulting engineer
Bion J. Arnold, consulting engineer
L. H. Baekeland, research chemist
Howard E. Coffin, vice president, Hudson Motor Car Company
Alfred Craven, consulting engineer, Public Service Commission, New York City
Thomas A. Edison
W. L. R. Emmett, mechanical engineer, General Electric Company
Peter Cooper Hewitt, inventor
A. M. Hunt, consulting engineer
M. R. Hutchison, Edison Laboratories
B. G. Lamme, chief engineer, Westinghouse Electric & Manufacturing Company
Hudson Maxim, inventor
Spencer Miller, Lidgerwood Manufacturing Company
J. W. Richards, Lehigh University
A. L. Riker, vice president, Locomobile Company of America
Thomas Robins, president, Robins Conveying Belt Company
M. B. Sellars
E. A. Sperry, Sperry Gyroscope Company
F. J. Sprague, consulting engineer
B. B. Thayer, vice president, Anaconda Copper Mining Company
A. J. Webster, Clark University
W. R. Whitney, director, Research Laboratory, General Electric Company
R. S. Woodward, president, Carnegie Institution of Washington

These members were the appointees or elected representatives of the following organizations: American Chemical Society, American Institute of Electrical Engineers, American Mathematical Society, American Society of Civil Engineers, American Aeronautical Society, Inventors' Guild, American Society of Automotive Engineers, American Institute of Mining Engineers, American Electrochemical Society, American Society of Mechanical Engineers, American Society of Aeronautic Engineers.

The War Committee of Technical Societies appointed D. W. Brunton after the formation of the Board. To facilitate its work, the Board divided itself into subcommittees, each of which handled a special problem.[8] The chiefs of the various bureaus into which the Navy is divided cooperated with the civilian board in order to render the latter's work more effective.

Just as the National Research Council became the "Department of Science and Research" of the Council of National Defense, so the Naval Consulting Board became its "Board of Inventions." The historian of the board tells us:

> By this arrangement there became a differentiation between scientific research and inventions, although inventions are frequently the outgrowth of scientific research, particularly where the arts have reached their highly developed state, as they had at the time of the declaration of war.

This statement must be borne in mind when we come to consider the relative accomplishments of the Naval Consulting Board and the National Research Council. The official history of the Naval Consulting Board makes little mention of the existence of the National Research Council. Furthermore, in the introduction to that history, Secretary Daniels stated that some of the most

[7] L. N. Scott, *Naval Consulting Board of the United States* (Government Printing Office, 1920).

[8] These were: chemistry and physics (later divided into two committees: chemistry; physics); aeronautics, including aero motors; internal combustion motors; electricity; mines and torpedoes; submarines; ordnance and explosives; wireless and communications; transportation; production, organization, manufacture and standardization; ship construction; steam engineering and ship propulsion; life-saving appliances; aids to navigation; food and sanitation; public works—yards and docks. Later committees added were: fuel and fuel handling; metallurgy; optical glass; special problems.

important work of the Board must still be held confidential.

According to Doctor Millikan, in a private communication to the writer:

> The records [of the Research Council] up to the late summer of 1918 are a little confused. The confusion was in the *situation*, which never got sharply into the records, rather than the way such records . . . [appear]. Of the original organizing group, I was the only one who was on the ground practically all of the time. Doctor Hale was there in March and April [1917], doing the general organizing job, while I was working primarily with the Navy and the Army on the actual scientific problems of the war. He was Chairman of the Council. My title was Executive Officer of the Council and Chairman of the Physical Science Division. It was of course under this division that practically all the antisubmarine, meteorological and sound-ranging activities fell. During several months in the summer and fall—August through October 1917—I was the only one of the organizing group who was in the office. This accounts for the fact that so much of the early activity of the Council had to do with physics and engineering. However, this is quite natural, since in the present war [World War II] it is the physicists and engineers, mainly, who are in most demand.

One of the first and most important problems of war research was in connection with the submarine menace. Lord Northcliffe, during his visit to the United States in the spring of 1917, continually reiterated that the submarine problem was *the* problem of the war. Allied Europe was unable to continue fighting without the iron, oil and other supplies that America alone was able to furnish. During the spring, the greatly intensified German submarine offensive threatened to stop all effective transportation between America and Europe. In one month alone, April 1917, 900,000 tons of Allied shipping were sunk by German submarines. Quick action was essential. The depth charge had already been invented, and naval officers were in unanimous agreement that a submarine, if it could be definitely located, could be easily destroyed. A mission of seven American scientific investigators was sent to France and England early in May 1917. In return, an allied scientific delegation came to America to consult with our experts. According to Doctor Millikan's communication to me,

> [This mission] . . . arrived in May and had in its membership Sir Ernest Rutherford and a young Commander of the British Navy, representing England; Professors Fabry and Abraham, outstanding in physics and electrical engineering, . . . the Duc de Guiche, radio expert, and Professor Grignard, outstanding chemist, all representing France; and Dr. Abetti of Italy.
>
> This mission was accredited to the National Research Council, and as the acting Chairman of the Council it was my job, with the authorization of the Secretary of War and the Secretary of the Navy, to act as their host in this country and to organize, as I did, two conferences between this mission and a dozen or so Army and Navy officers and a like number of American physicists and engineers. The information brought to this country on European attempts at submarine detection and sound ranging was responsible for the initiation of American groups, mostly physicists, to deal with these fields.

Three groups of investigators were beginning work on this question under the Bureau of Steam Engineering of the Navy, and it was also a problem of study in both the Naval Consulting Board and the National Research Council. A conference was called in Washington, to which were invited the "best brains" in the field of physical science in America.[9]

The problem of locating a submarine was, as Sir Ernest Rutherford pointed out at the time, a problem of physics, pure and simple. And the Committee now appointed to work on it was therefore composed of physicists. It was not yet, as Doctor Millikan pointed out, "a problem of engineering . . . , although every physical problem, in general, sooner or later becomes one for the engineer, when the physicist has gone far enough along with his work." The antisubmarine committee consisted of Professors Merritt of Cornell, Mason of Wisconsin, H. A. Wilson

[9] Reference 7, p. 235. The invitees were: Rear Admiral W. S. Sims, USN; Capt. W. S. Smith, USN; Comdr. J. K. Robison, USN; Comdr. Yates Stirling, Jr., USN; Lt. Comdr. G. W. S. Castle, USN; Naval Constructor E. S. Land, USN; E. O'C. Acker, Bethlehem Steel Co.; P. W. Bridgman, Harvard University; A. P. Brush, General Motors Co.; W. H. Burr, Columbia University; L. S. Clarke, Auto Car Co.; G. H. Condict, electrical engineer; F. G. Cottrell, chief physicist, Bureau of Mines; G. C. Davison, Electric Boat Co.; F. I. du Pont; R. A. Fessenden, Submarine Signal Co.; H. W. Fisher, Standard Underground Cable Co.; H. C. Ford, Ford Instrument Co.; J. R. Freeman, consulting engineer; C. F. Kettering, Dayton Electrical Laboratories; A. Kingsbury, mechanical engineer; M. S. Kintner, National Electric Signaling Co.; I. Langmuir, General Electric Co.; F. Leavitt, E. W. Bliss Co.; A. R. Ledoux, consulting engineer; L. Lyndon, consulting engineer; R. D. Mershon, consulting engineer; A. A. Michelson, University of Chicago; R. A. Millikan, University of Chicago; E. F. Northrup, Princeton University; G. W. Pierce, Harvard University; R. H. M. Robinson, Lake Torpedo Boat Co.; C. F. Scott, Yale University; B. Speed, Western Electric Co.; J. A. Steinmetz, Janney & Steinmetz; H. B. Smith, Worcester Institute of Technology; H. R. Sutphen, Submarine Boat Corp.; P. H. Thomas, consulting engineer; E. Thompson, General Electric Co.; R. B. Williamson, Allis Chalmers Co.; R. W. Wood, Johns Hopkins University.

of the Rice Institute, Pierce and Bridgman of Harvard, Bumstead, Nichols and Zeleny of Yale, and Michelson of Chicago (who never served, since he was immediately reassigned to other work of great urgency, and was replaced by Millikan, who organized the group). The official name was The New London Experiment Station. According to Doctor Millikan:

> [It] was organized under the authority of Admiral Griffin, Navy Chief of Engineers, at my request. He authorized me to select the men for the station and send them wires to assemble at New London. I picked out the above group and wired them to meet me at the Mohican Hotel in New London on the evening of July 4. I carried to that meeting all the information brought to me by the European mission, and we spent most of the night analyzing it and assigning problems.

Submarine-detection devices are of two principal types: listening devices, and acoustic instruments analogous to searchlights. A simple stethoscope, the kind a physician applies to a patient's chest, can be placed under water and connected to the ear by tubing. Such a device will render audible the sounds emanating from a submarine moving rapidly at a distance of more than a mile way. Two stethoscopes, at opposite ends of a bar 3 or 4 ft long, enable one to determine the direction of a moving submarine by rotating the bar in a horizontal plane and making use of the same binaural discrimination that ordinarily enables us to ascertain the direction of sounds with our ears alone. Such a device on a large scale was developed by the antisubmarine research group. Although Professor Mason, of the University of Wisconsin, was largely responsible for the final form of the device, this form was a variant of a less satisfactory French device which had been demonstrated at the Allied scientific conference held in Washington in the spring of 1917. Mason's instrument was capable of detecting moving submarines at a distance of 10 mi and was used on many American submarines and destroyers that went across during the summer of 1918.

The listening device just described had several defects, however, and another type was developed, one making use of the well-known fact that a beam of sound waves can penetrate water to a great distance without being quenched, as light would be by a short thickness of water. The most useful frequencies for this work proved to be those in the range from about 10,000 to 100,000 c/sec. A device was developed by means of which this supersonic beam was sent out and its echo, when detected, would locate the undersea craft. However, this device did not then come into practical use. Before either of these devices was applied on a sufficiently large scale to combat the menace of submarines effectively, the convoy system, at first regarded by masters of merchant ships as impracticable, was actually applied successfully.[10]

Improved detection devices of both sorts are now in use. Like many other products of the last war, both found important peacetime applications. The first type was altered to enable ship captains to prevent collisions during fog, but in recent years a more efficient means of accomplishing the same end has been developed by making use of the cathode-ray oscilloscope. The second type was used extensively as a simple and very accurate way of taking depth soundings, though for this purpose it is the audible range of frequencies that is usually used.[11]

Research in physics[12] was under the direction of the Executive Committee of the Division of Physical Sciences of the National Research Council. The Committee's membership included representatives of important scientific and technical agencies, as follows:

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[10] These devices and their history are described in references 7 and 12.

[11] Various developments along the line of submarine detection were made by members of the Naval Consulting Board and their affiliates: W. R. Whitney, I. Langmuir, W. D. Coolidge, F. B. Jewett, H. W. J. Fay, B. G. Lamme, Maj. R. D. Mershon, C. F. Scott, V. Bush, J. B. Whitehead, L. O. Grondahl, E. F. Nichols, J. Zeleny, and others. See reference 7, Chap. 4, for a description of their work. In the same place there is an account of the work done at the Nahant station, and the work done by various industrial firms, such as the General Electric Co., the Western Electric Co., and the Submarine Signal Co.

[12] The most detailed account of activities of physicists during the war may be found in R. M. Yerkes, *The new world of science, its development during the war* (Century, 1920). This is a composite volume, each chapter being written by a different person. The most interesting ones from the point of view of physics are: Millikan, "Contributions of physical science" and "Some scientific aspects of the meteorological work of the United States Army;" Trowbridge, "Sound-ranging in the American Expeditionary Forces;" Ives, "War-time photography;" Howe, "Optical glass for war needs;" Kennelly, "Advances in signalling contributed during the war." See also N. M. Hopkins, *The outlook for research and invention* (Van Nostrand, 1919); ch. iv is devoted to "American war research."

The complete 1917 wartime organization of the NRC, including a list of chief personnel, may be found in Report of the National Research Council 2, 57–62 (1917). The subcommittees in physics included: submarine investigations (9 groups); wireless; location of invisible aircraft; location of mining and tunneling operations; muzzle velocity of projectiles; naval range finders; range finders for antiaircraft service; static charges on airships; submarine mines; use of high pressure in manufacturing guns; visibility; monoculars and binoculars; camouflage.

J. S. AMES, Johns Hopkins University, representing the National Advisory Committee for Aeronautics
L. A. BAUER, Department of Terrestrial Magnetism, Carnegie Institution of Washington
A. L. DAY, Geophysical Laboratory
A. L. LEUSCHNER, Chemical Warfare Service
C. F. MARVIN, Chief of the Weather Bureau
R. A. MILLIKAN, representing the Signal Corps and the Antisubmarine Board of the Navy
F. R. MOULTON, Ordnance Department of the Army
C. E. MENDENHALL, Bureau of Aircraft Production
E. F. NICHOLS, Bureau of Ordnance of the Navy
H. N. RUSSELL, associated with both the Engineer Corps and the Bureau of Aircraft Production
W. C. SABINE, of the Advisory Committee for Aeronautics and the Bureau of Aircraft Production
FRANK SCHLESINGER, Bureau of Aircraft Production
GEORGE O. SQUIER, Chief of the Signal Corps
S. W. STRATTON, Director of the National Bureau of Standards
R. S. WOODWARD, President of the Carnegie Institution of Washington

This was one of the subsidiary groups to the Administrative Division, which not only directed the activities of the Council, but also was in charge of relations between American and Allied scientists. Branch offices were established in London and in Paris, and many American scientists did scientific research abroad. The Administrative Division was in charge of the census made of research facilities so that problems might be assigned as they arose. It also supervised relations with the Patent Office and directed the activities of the Research Information Service.[13]

The exchange information service had its inception in the spring of 1917, when a group of English scientists of the British Ministry of Munitions addressed a letter to General George O. Squier, Chief of the Signal Corps, suggesting the development of better liaison between British and American scientists. General Squier referred the letter to Doctor Millikan, and the National Research Council drew up a comprehensive plan for bringing about full cooperation among the allied scientific groups and for preventing unnecessary duplication of effort. Four new offices were established: in Washington, London, Paris and Rome. The importance of the Research Information Service may be seen from the fact that the Washington office was directed by the chairman of the National Research Council, the chief of the Army Intelligence Service and the chief of the Navy Intelligence Service. As it turned out, the service had most to do with developments in the physical sciences; hence, Millikan's active connection with it. His wartime career is indicative of the many services performed by individual American scientists. At one and the same time, he was one of the original founders of the NRC, chairman of the Division of Physical Sciences within NRC, a Lieutenant Colonel in the Signal Corps, and an official of the Antisubmarine Board of the Navy.

Through the agency of the exchange service, uncensored reports of war research were sent back and forth among the four offices. At the request of the General Staff of the Army, the Secretary of War issued orders to all army officers on scientific and technical missions to make duplicate reports, one to go to the Army proper and the other to the civilian attaché to be forwarded to a central agency so that an interconnection might be established between new developments as they came along. Each week there was held in Washington a conference of the Divisions of Physical Sciences and of Engineering of the NRC. Here the accumulated reports of the week were scrutinized, discussed, abstracted and then passed along to the groups of scientists who needed that information for their research. For example, all antisubmarine work done in England, France and Italy was assembled by the Research Information Service's staff in London, Paris and Rome, respectively, and then reported by cable or uncensored mail to the Washington office. From there it was sent each Saturday night in a concise or "predigested" form to the scientists working on antisubmarine research at New London and Nahant. Thus the work of the NRC progressed with extraordinary rapidity for two reasons: there were formed a considerable number of highly competent research groups, and there were established effective channels for full cooperation among them.

Other work done by physicists included the development of sound-ranging devices of various sorts. Some of these devices were used to detect

[13] Dr. Millikan, in a letter to the writer, said: "In the few years immediately following the war, the Research Information Service, headed by Robert Yerkes, was an attempt to keep American industrial and research groups informed as to the research personnel of the country and the status of research developments. This was found so grandiose and difficult an undertaking that it was abandoned after perhaps the fifth year. It is entirely possible, as your review indicates, that in the first few months of the war we called our exchange of information between this country and our allies 'The Research Information Service.' . . . If so, the Research Information Service in wartime was something quite different from what those of us who were associated with the National Research Council became accustomed to thinking about under that designation in the late war and postwar period, but it is entirely possible that there was an evolution of function. . . ."

airplanes; others to locate the position of enemy artillery posts. Some of the new instruments made use of the principle of binaural addition, or discrimination, referred to earlier. This phenomenon, curiously enough, was practically unknown to most physicists before the war. Yet all of us use it unwittingly from day to day: when we wish to determine the direction of the origin of a sound we turn our head until we are *looking* in the direction from which we think the sound comes.

But by far the most widely used devices were those for locating enemy artillery posts and also tunneling operations of the enemy. Artillery was located by the method of flash ranging and also by sound ranging. This service was organized by Professor Augustus Trowbridge, of Princeton University, assisted by Theodore Lyman, of Harvard University. Sound-ranging equipment used was of British design; flash-ranging equipment, of French design. The accuracy of the locations was surprisingly high during the eight months of operation, from March to November 1918. Army topographers made surveys of the gun positions which had been located by sound ranging after the capture of the St. Mihiel salient, and the errors discovered were corrected for in later work. After the Armistice, another check was made; it was found that the error was often less than 30 ft and rarely more than 70 ft. Trowbridge reported, after the war was over, how much ranging, both sound and flash, had been used. During one period of very rapid advance, 425 locations of enemy batteries were made; of these two American flash-ranging sections observed 63 percent, three French flash-ranging sections observed 16 percent, and three American sound-ranging sections observed 21 percent. Professor Lyman tells me, however, that although much information was obtained by flash and sound ranging, it was not always used; this depended upon the amount to which the commanding officer of artillery intelligence was willing to cooperate.[14]

Supersonic signaling under water had been developed first in France, notably under the direction of Paul Langevin. We have seen its further uses in submarine detection. Two other signaling devices were invented by Americans. One used infra-red light. Developed by Theodore Case, of Auburn, New York, this system of invisible signaling successfully solved the problem of keeping convoys together at night without showing lights, which would betray their position to enemy submarines. Another system, developed by R. W. Wood, used ultraviolet light. The latter device enabled aviators to find their way back to landing fields at night. Ultraviolet light was also used to detect passport forgeries.[15]

Physicists aided in the development of the meteorological service of the Army and in the perfection of ballooning technics. They contributed to the perfection of a leak-proof gasoline tank for airplanes. They worked on the problem of supplying optical glass for military purposes,[16] a material which had hitherto been imported entirely from Germany. And they built one of the finest aerial cameras to be used during that war.

The importance of the work done by the Division of Physics and of Engineering was greatly increased in 1917 when the National Research Council was requested by the Chief Signal Officer of the U. S. Army to organize the Division of Science and Research of the Signal Corps. The problems to be solved were of various kinds. One, for example, was the improvement of aerial photography, work on which was undertaken by a group of physicists and photographic experts under the direction of Herbert E. Ives. Color filters for detecting camouflage and for increasing visibility were developed by this group, and also new dyes for making panchromatic plates, useful not only for their panchromatism but for the high speed of their emulsions. The new methods of photography revolutionized military surveying, since an airplane photograph made in but a few seconds could give most of the essential information previously obtainable only at the cost of several weeks of triangulation.

Another group worked on the application to wireless communication of the grid triode vacuum tube ("audion"), invented by Lee De Forest. The triode had already proved its value to the telephone industry, whose physicists had successfully applied its amplifying characteristics to telephony, thereby increasing the possibilities of

[14] A vast literature exists about this subject. The best short account is Trowbridge's in reference 12 (Yerkes). For an account of the actual day-to-day job of ranging, see J. R: Hinman, *Ranging in France with flash and sound* (Dunham Printing Co., 1919).

[15] Described in reference 12 (Yerkes). See also two articles by R. W. Wood on the uses of ultraviolet light in warfare: J. de Phys. **9**, 77 (1919); London Physical Soc. Proc. **31**, 232 (1919). See also W. Seabrook, *Doctor Wood, modern wizard of the laboratory* (Harcourt Brace, 1941), pp. 178 ff.

[16] Work on the production of optical glass was done jointly by a Division of the NRC, a group working under the Naval Consulting Board, the Geophysical Division of the Carnegie Institution of Washington, and various industrial groups.

communication (for example, feasible long distance service) and at the same time making possible a saving of many millions of dollars. Another problem involved the development of plane-to-plane communication. This problem, presented in April 1917, was solved in midsummer by the physicists of the Western Electric Company to whom it had been referred. Production began immediately, and, on the first Sunday after Thanksgiving of the same year, airplanes in flight were, for the first time in history, directed by the flight commander in the leading airplane and also from the ground. Reports and directions were "received in clear speech."

The activities of American scientists during the World War had a great effect upon the organization of science itself. Prior to the war and the formation of the National Research Council, scientific research in the United States had been carried on, for the most part, by groups or agencies working independently of one another. Research in "pure" science had been chiefly confined to universities and privately endowed institutions of research. The Government bureaus, industrial laboratories (with a few notable exceptions), and the various State scientific agencies had been concerned with practical problems. But, under the agency of the National Research Council, all the manifold activity of scientific research had been welded into one compact and effective unit. Scientists had come into contact with the work of other fields than their own, and no one will deny the great value of this experience in the postwar period. Industrialists gained the rich experience of working with scientists, and this created a great boom in industrial research after the war. In fact, large-scale industrial research, as we know it today, largely grew out of the experience in the NRC. Furthermore, the public at large had come to recognize to a much greater extent than hitherto the supreme importance of science in the conduct of affairs of daily life.

The most vivid picture of American science as it emerged from the World War was presented in 1920 by George Ellery Hale, who was probably in a far better position than anyone else to see the true condition of science, its needs and its possibilities for the future. He wrote:

> No one can survey the part played by science in the war without reflecting on the ultimate influence of the war on science. Able investigators have been killed or incapacitated, and with them a host of men who might have taken high places in research. Sources of revenue have been cut off, and the heavy financial burdens permanently imposed upon individuals, institutions and governments must tend to reduce the funds available for the advancement of science.
>
> On the other hand, the usefulness of science is appreciated as never before, and some newly enlightened governments have already recognized that large appropriations for research will bring manifold benefits to the state. The leaders of industry have also been quick to appreciate the increased returns that research renders possible, and industrial laboratories are multiplying at an unprecedented rate. The dearth of available investigators and the higher salary scale of the industrial world have seriously affected educational institutions, members of whose scientific staffs, inadequately paid and tempted by offers of powerful instrumental equipment, have been drawn into the industries. On the other hand, industrial leaders have repeatedly emphasized the fundamental importance of scientific researches made solely for the advancement of knowledge, and the necessity of basing all great industrial advances on the results of such investigations. Thus they may be expected to contribute even more liberally than before to the development of laboratories organized for work of this nature.
>
> Educational institutions are also likely to recognize that science should play a larger part in the curriculum, and that men skilled in research should be developed in greatly increased numbers. The enlarged appreciation of science by the public, the demand for investigators in the industries, and the attitude of industrial leaders of wide vision toward fundamental science, should facilitate attempts to secure the added endowments and equipment required.[17]

Certainly, not the least remarkable aspect of this statement is the fact that it might very well have been written today rather than 25 years ago.

## Between Wars

In the midst of World War II, American physicists and other scientists can take comfort as well as pride in the accomplishments of American science since 1920. Americans in every branch of scientific activity took their place among their most distinguished world colleagues. American universities progressed so rapidly that their claim to supremacy is now undisputed. The general picture of expansion is nowhere reflected better than in the membership rolls of the American Association for the Advancement of Science. From 1920 to 1940, this membership almost doubled; it increased from 11,547 to 21,150. At the same time, American industrial research grew by leaps and bounds. The National Resources Committee tells us that during these two decades:

[17] From an essay by Hale in reference 12 (Yerkes), p. 393.

(1) The number of industrial research laboratories increased from about 300 to more than 2200.

(2) Scientific personnel in industry increased from approximately 9300 to more than 70,000.

(3) The number of companies employing research staffs of more than 50 persons increased from 15 to 120.

After the war, the National Academy of Sciences and the National Research Council played the new role implicit in President Wilson's statement to the Council of National Defense that the task was to unite all the forces of the country "for the victories of peace as well as those of war." This attitude is further amplified in a prophetic statement of George Ellery Hale's on the national importance of scientific research, in which he declared:

> If war now dominates the [scientific] field in England and in France, there are men and organizations in both countries who recognize that a greater trial may come later, when the united industries of Germany, surprisingly developed by scientific research, will fight a fierce industrial war in the markets of the world.[18]

Such considerations were surely in President Wilson's mind on May 11, 1918, when he issued the executive order requesting the Academy to perpetuate the NRC, in order to utilize its "capacity for larger service," so well demonstrated in the solution of war problems. This order constituted an inclusive charter of purposes, summarized in two of its paragraphs as follows:

> 1. In general, to stimulate research in the mathematical, physical and biological sciences, and in the application of these to engineering, agriculture, medicine and other useful arts, with the object of increasing knowledge, of strengthening national defense and of contributing in other ways to the public welfare.
>
> 2. To survey the larger possibilities of science, to formulate comprehensive projects of research, and to develop effective means of utilizing the scientific and technical resources of the country for dealing with these projects.

The NRC's activities between wars[19] can be conveniently divided into three classes: training of scientists, fostering of industrial research, and the advancement of scientific knowledge. In 1920, the Rockefeller Foundation gave the Council $100,000 for subsidization of fellowships; similar grants were made in succeeding years. These postdoctoral fellowships were awarded to young scientists at the most productive period of their lives, and the condition that the National Research Fellow spend his time at an institution other than the one at which he did his graduate work made him the carrier of new ideas and technics from one institution to another.

In addition to its work in furthering industrial research, the Council subsidized the publication of scientific papers, prepared abstracts and bibliographies of current research, authorized and financed the preparation of numerical tables and collections of scientific data, and sponsored a series of reports on the status of certain theoretical and experimental aspects of contemporary science, which not only clarified the problems at hand but served as textbooks for both the advanced student and the practising scientist. Its little brochures on fields of research are responsible for the choice of a career of many of our younger scientists.

The annual reports of the Council's Division of Federal Relations give evidence of the willingness of the membership of the NRC to serve the government directly, although there was little call on their services. In 1926, the National Academy appointed a Committee on Government Relations "to study the relation of the Academy to the greater problems as set up by the Government," a committee that remained largely inactive until 1933.

It would be false, however, to give the impression that science performed no *direct* services for the Government in this period. There were scientists working in the laboratories of the various Government bureaus. Many others were busy in the laboratories and proving grounds of the Army and the Navy. The National Advisory Committee on Aeronautics was laying solid foundations for the development of American aviation. This committee had been appointed by President Wilson in 1915; its membership consisted of two representatives each from the Navy and Army, one each from the Smithsonian Institution, the Weather Bureau, and the National Bureau of Standards, and "not more than five additional persons who shall be acquainted with the needs of aeronautical engineering or its applied sciences." At the end of the war, the Committee reserved to itself the right to initiate research which it considered needful and timely for aeronautics, and it retained its original and primary task of furnishing information desired by any agency of the Government. As early as 1919 it uttered warnings for which the times,

[18] "The national importance of scientific research," Tech. Rev. **18**, 801–817 (1916).

[19] Described in the annual Report of The National Research Council.

unfortunately, were not ripe. Its *Report* for that year said:

> From the lessons of the war we know that aeronautics will be the first arm of defense and of offense to come into action in future wars. Victory will sharply incline to that side that establishes superiority in the air. . . . The Committee invites the attention of Congress to the need for providing encouragement for the development of commercial aviation, as well as military aviation, and to the need for more liberal support of research and experimental work in aeronautics.

In 1926, the Committee sponsored the first of its annual Aircraft Engineering Research Conferences, and in that year Congress passed the Air Commerce Act, long advocated by the Committee.

The great depression had its effect on scientists as well as on nonscientists. This may be seen in the creation of the Science Advisory Board, appointed in 1933 by President Roosevelt, which proposed a National Program for Putting Science to Work for the National Welfare. This board, widely supported by scientists, recommended the establishment of a permanent science advisory board whose members would be appointed by the President on the nomination of the National Academy of Sciences. The board would, in the words of the report, "bring to the Department Secretaries, or to the President, any recommendations which, in its judgment, will improve the effectiveness or efficiency of the scientific work of agencies of the Federal Government." The board asked for an appropriation of $1,750,000 a year for two years, to be distributed by the National Research Council as grants-in-aid to nonprofit institutions under the guiding principle that projects most useful for "the advancement of science and of the public welfare" be given preference. The recommendations were not heeded. A similar job was done by the National Resources Planning Board, which appointed a special science committee. Nothing tangible came out of this work either.

In 1935, the Academy established a Governmental Relations and Science Advisory Committee. President Roosevelt thereupon circulated a memorandum to all scientific agencies of the Government, apprising them of the aid available to them from the Academy. Many departments were quick and ready to accept this aid. At the request of the Secretary of the Navy, a committee was appointed to study the design and construction of warships. Other projects of this sort include: research on soil conservation, studies of the relation of the patent system to the stimulation of new industries, signaling at sea, and Air Corps problems.

As the years of the present war approached, then, the relations between American scientists and the Government had become more closely knit than hitherto. The Army and Navy scientists continued their work in secret and without applause. The National Advisory Committee on Aeronautics had all the while pursued its main function, "the scientific investigation of fundamental problems," as it was so fond of reiterating. In 1936, its constant pleas for more funds, facilities and personnel began to take on a tragically unnoticed insistence. As late as 1938, the Committee, contrasting our status with that of Germany (and other European nations), was forced to admit: "American leadership is now threatened by the great expansion of research facilities in other nations."

In 1939, on the eve of its greatest task, the Committee suffered the loss of its chairman, Joseph Sweetman Ames, whose career of public service is one of the most notable in the annals of American science.[20] A distinguished physicist, Doctor Ames was professor of physics and later President (1929 to 1935) of the Johns Hopkins University. An original member of the Committee, he served as its chairman continuously for 24 years until his resignation. So remarkable a man did not finally go unhonored by his fellow citizens. In the letter accepting his resignation, President Roosevelt said:

> Our republic would not be worthy of the devoted service you have rendered for twenty-four years without compensation if it could not on this occasion pause to pay tribute where it is so justly due. . . . The administration and the accomplishments of the Committee under your leadership reflect your great scientific attainments, professional courage, and executive ability.[21]

In 1940 the new research laboratory of the Committee at Moffett Field was named in honor of Ames.

In the difficult times that lay ahead, the American Government and the American people could count on such men as Ames. It could count on the aid of a large and well-trained body of scientists who recognized the duty and the privilege of service to their country.

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[20] See N. E. Dorsey, "Joseph Sweetman Ames: the man," Am. J. Physics **12**, 135 (1944).

[21] All members of the NACA serve without compensation.

## The Present Conflict

In the middle of 1941, months before the attack on Pearl Harbor, a survey made by Henry A. Barton, Director of the American Institute of Physics, showed that one out of every four physicists was already working for national defense.[22] By the end of that year, 75 percent of our most distinguished physicists (those "starred" in ***American Men of Science***) were so engaged. According to the American Institute of Physics, there were 6400 trained physicists in America at that time, of whom 2500 were employed by industry. We did not, therefore, come poorly prepared to a war which has been frequently referred to as a "war of physics," in the same sense that the last war was referred to as a "war of chemistry." In addition to such persons, we had a vast reservoir of technicians trained in the very skills so necessary to the present type of warfare—from such sources as our electrical, radio and telephone industries.[23]

During the years of peace, the Army and the Navy had maintained research laboratories, such as the Chemical Warfare Laboratory at Edgewood, the Naval Ordnance Laboratory, the Picatinny Arsenal, the Aberdeen Proving Ground, Wright Field, the Naval Research Laboratory, and many others. In the first years of the war in Europe, these research centers expanded and continued their excellent work. Likewise, the National Advisory Committee for Aeronautics continued with its program, which became of ever-increasing importance.

Some day, perhaps, when this war is over, the country will learn something of the activities of the groups just mentioned. Surely, not the least significant aspect will be how much was done with so little in the way of funds, equipment and personnel. Even before Germany attacked Poland, it was clear to many scientists and military men that any future war would call more heavily on the scientific resources of a nation that hoped to achieve victory than the World War did. Frank B. Jewett, President of the National Academy of Sciences, tells us:

> As soon as war in Europe on a vast scale was seen to be imminent, the nations there commenced frantically to mobilize and organize their scientific and technical men and resources, and to establish effective liaison between them and the combat services. For more than a year after this movement was in full swing across the Atlantic, our aloofness from the struggle and our ardent desire to keep from being sucked into the tragic maelstrom operated to prevent any effective steps in the direction of mobilizing our vast scientific resources for total war. The military services endeavored to strengthen their scientific branches, and here and there enlisted the aid of civilian science. They were hampered by inadequate funds, by the pattern of years of a starved organization imposed by an antiwar philosophy, and by the fact that civilian sciences, both fundamental and applied, were built up on a basis of operation in a slow-moving peace economy. The latter had no machinery for marshaling its forces for war and, in the main, it knew little of war's requirements and frequently preferred to follow the courses it understood and liked.[24]

Quite naturally, the first group from which aid was enlisted was the National Academy of Sciences and its instrumentality, the National Research Council. In October 1940, Doctor Jewett reported:

> Beginning about two years ago, requests began to peak up in accelerated fashion, and during the past eighteen months the Academy, either directly or through its agent, the National Research Council, has been called upon to advise, study and report on a wide variety of matters, in which the actual costs paid by government under the charter are running at the rate of a number of hundred thousand dollars a year. [These do not include any remuneration to members or their appointees on committees.]
>
> These requests for study and advice have come from the President, from numerous branches of the Army and Navy, and from many of the civil departments, boards and commissions, both permanent and temporary.[25]

To indicate the scope and variety of the problems submitted to the Academy, some handled by committees of the Academy alone, more by committees appointed jointly by the Academy and Council, Doctor Jewett selected "at random" five typical ones as follows:

> 1. From the President of the United States—Study and report on a method of instrument (blind) landing for aeroplanes to be standardized and employed at major flying fields.
>
> 2. From the Civil Aeronautics Authority—Comprehensive study of the whole method of selection and training of pilots.

[22] H. A. Barton, Rev. Sci. Inst. **12**, 525 (1941).

[23] See: J. A. Crowther, "Physicists and the war," J. App. Physics **12**, 767 (1941); H. L. Dodge, "War training in physics," Am. J. Physics **10**, 50 (1942); editorial, "Maintaining the supply of physicists," J. App. Physics **12**, 711 (1941); J. C. Morris, "An appeal for physics graduates," Am. J. Physics **9**, 381 (1941); editorial, "Physicists and national defense," Science **93**, 275 (1941); I. H. Solt, "Training physicists for defense industries," Am. J. Physics **9**, 294 (1941); editorial, "The 'supply' of physicists," J. App. Physics **12**, 655 (1941).

[24] F. B. Jewett, "The mobilization of science in national defense," Science **95**, 235 (1942).

[25] F. B. Jewett, Science **92**, 414 (1940).

Courtesy, *The Technology Review.*

Dr. Vannevar Bush.

3. From the National Resources Planning Board—Study and report on industrial research in the United States.

4. From the Advisory Commission to the Council of National Defense—(*a*) Study and report on the whole problem of utilizing domestic low-grade manganese ore; and (*b*) study and report on the erection of a tin smelter to re-refine low-grade tin ores from Bolivia.

5. From the Army and Navy—Numerous problems concerned with national defense.

Nevertheless, there was need for a civilian group with executive powers to supplement the scientific and technical work of the Army and Navy, since, as Dr. Vannevar Bush tells us, "an expansion of the scientific attack on war problems was essential, and the Army and Navy could not themselves undertake this immediately and fully."[26] The Academy and the Council, it must be borne in mind, are advisory bodies to the government, rather than executive bodies with power to act.

In June 1940, therefore, a new agency was created: the National Defense Research Committee, familiarly known simply as NDRC. Its origin is described for us by Doctor Jewett:

[26] V. Bush, "Science and national defense," Science **94**, 572 (1941).

> When . . . the group [interested in this problem] . . . became convinced that broader participation by civilian scientists in the whole military program was likely to be essential, they regarded the N[ational] A[dvisory] C[ommittee for] A[eronautics] as typifying the sort of organization they would like to see created. A plan was therefore drawn up envisaging a Committee composed in part of civilian scientists and in part of Army and Navy representatives. On the one hand, the Committee was charged with a broad study of the materials of warfare and, on the other, it would recommend and, if possible, initiate such research as it believed to be in the national interest.[24]

The new body was established by executive order through the medium of the Council of National Defense, of which it became a subordinate body. Its membership included the following persons:

Vannevar Bush, chairman
James B. Conant
Richard C. Tolman
Karl T. Compton
Frank B. Jewett (as President of the National Academy)
Conway P. Coe (as Commissioner of Patents)
H. G. Bowen, Rear Admiral, USN
G. V. Strong, Brigadier General, USA.[27]

The Order establishing the NDRC stated that:

> The Committee shall correlate and support scientific research on the mechanisms and devices of warfare, except those relating to problems of flight included in the field of activities of National Advisory Committee for Aeronautics. It shall aid and supplement the experimental and research activities of the War and Navy Departments; and may conduct research for the creation and improvement of instrumentalities, methods, and materials of warfare. In carrying out its functions, the Committee may (*a*) utilize, to the extent that such facilities are available for such purposes, the laboratories, equipment and services of the National Bureau of Standards and other Government institutions; and (*b*) within the limits of appropriations allocated to it, transfer funds to such institutions, and enter into contracts and agreements with individuals, educational or scientific institutions (including the National Academy of Sciences and the National Research Council) and industrial organizations for studies, experimental investigations, and reports.

By October 1941, about 1000 scientists were working in academic institutions under contracts with the Committee. More than 700 additional scientists were similarly employed in industrial firms. Just before our entry in the war, the total had risen to some 2000, of whom 500 were working directly with the Committee and the remainder under its research contracts.[24]

[27] Reference 24. This was the organization at the time of our entrance into the war.

The scope of the Committee's activities is evident in the number of contracts for research. In its first year of operation, the Committee initiated research under more than 400 contracts, the parties to which included about 75 educational institutions and 50 industrial organizations. The sum of $15,000,000 had been expended up to October 1941.[24] The Committee also undertook collaboration with British scientists. In March 1941, President Conant spent several weeks in England organizing British-American scientific liaison.

At the time of our entrance into the war, there were four major departments of NDRC: Division A, with Dr. Richard C. Tolman of the California Institute of Technology as chairman, dealing with armor, bombs and ordnance in general; Division B, headed by Dr. Roger Adams of the University of Illinois, dealing with chemistry; Division C, headed by Doctor Jewett, dealing with transportation and communication, and submarine warfare; Division D, headed by Dr. Karl T. Compton, President of the Massachusetts Institute of Technology, on "instruments and numerous miscellaneous projects difficult to catalog." The subsurface warfare laboratories were operated under Division C, and the microwave laboratory was organized under Division D.[25]

During the first year of operation it became evident that a broader program of attack on the biological and medical sciences (omitted in the order creating NDRC) not only would be useful but was, in reality, urgently demanded. Thus, in June 1941, President Roosevelt created a new body, the Committee on Medical Research, to explore its indicated territory in the same manner that NDRC had been exploring the physical sciences. Then, over and above both NDRC and the new CMR, there was placed a supreme coordinating body, the newly established Office of Scientific Research and Development, known as OSRD. The latter office was placed in charge of Doctor Bush, who previously had been head of NDRC. President Conant was then made head of NDRC and Dr. Newton Richards of the Medical School of the University of Pennsylvania was made Chairman of CMR.[28]

The order creating OSRD stated that its advisory council would be composed of the Director, as chairman, the Chairman of the National Advisory Committee on Aeronautics, the Chairman of NDRC, the Chairman of CMR, a representative of the Army and a representative of the Navy. OSRD, like NDRC, is a temporary, or emergency, organization. It has certainly won the respect of Army and Navy officials and has proved its great efficiency for war time.[29] Quite naturally, then, President Roosevelt asked the director of OSRD what we may learn from the experience of this war that will be of value to science and to the community once the war is won. As we read the reply of Doctor Bush to President Roosevelt's letter with great interest,[30] and wonder along with President Conant: "Is it fantastic to hope that in the not too distant future the scientists of all free countries may be joined in an effective action to improve not instruments of war, but those of peace?"[31]

*(This is the second of two articles on American Physicists at War)*

[28] In addition to references 24 and 26, some other sources of information concerning NDRC and OSRD are: (1) "Adventures in science," mimeographed excerpt from a radio program over the Columbia Broadcasting System, Apr. 10, 1941, in which Irvin Stewart of NDRC and Watson Davis of Science Service discussed "the question of how scientific research is aiding the national defense effort;" (2) K. T. Compton, "Technological and scientific resources," Annals Am. Acad. Political Sci. **218**, 66 (1941); (3) K. T. Compton, R. W. Trullinger and V. Bush, *Scientists face the world of 1942* (Rutgers Univ. Press, 1942); (4) J. B. Conant, "University training and war service in Great Britain," Assoc. Am. Colleges Bull. **27**, 372 (1941); (5) J. B. Conant, "The National Defense Research Committee," Harvard Alumni Bull. **44**, 56 (1941); (6) J. B. Conant, letter to the editors, Life **11**, 2 (1941); (7) "Electricity and defense: what electrical research means to defense," mimeographed account of a radio program presented March 4, 1941, over the Blue Network, National Broadcasting System, by F. B. Jewett, K. T. Compton and J. W. Barker; (8) L. A. Hawkins, "Engineering and research for American defense," Elec. Eng. **59**, 355 (1940); (9) W. B. Kaempffert, "American science enlists," New York Times Magazine Section (Nov. 2, 1941); (10) "Technological high command," Fortune **25**, 63 (1942); (11) editorials and news items in *Science*, *American Journal of Physics*, *Review of Scientific Instruments*, *Journal of Applied Physics*, *Bell Telephone Journal*, *Electronics*, etc.; (12) mimeographed press releases of NDRC; (13) executive orders creating and altering NDRC and OSRD, reprinted in *Science* and the public press and distributed in mimeographed form; (14) G. W. Gray, *Science at War* (Harpers, 1943).

[29] Other wartime groups in whose activities physicists have been concerned but which have not been discussed here include: The National Roster of Scientific and Specialized Personnel, The Engineers' Defense Board, The National Inventor's Council, and various groups organized to expedite training in physics and engineering.

[30] V. Bush, *Science the endless frontier, a report to the president, July 1945* (United States Government Printing Office, 1945).

[31] *Final note.* Although the bibliographic references in these two articles give the sources of the chief statements made in the text, no attempt has been made to cite all of the vast literature on this subject. The writer hopes that readers who have corrections or emendations to any portion of these articles will send them to him so that he may collect them for future publication in this journal and thus make the published record accurate and more complete.

# Reproductions of Prints, Drawings and Paintings of Interest in the History of Physics

## 22. Il Tempio Voltiana in Como

E. C. Watson
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The 200th anniversary of the birth of Alessandro Volta (1745–1827) occurs this year. It is proposed to commemorate this bicentennial by publishing a brief description of the sumptuous temple erected in his honor at his birthplace, Como, Italy. A description of a similar memorial to an even greater Italian physicist appeared earlier in this series.[1] What other nation has so signally honored its great scientists?

The "Volta Temple," a white marble building which was completed in 1927, is situated in public gardens close to the shore of Lake Como. As Plates 1 and 2 show, it is in the neoclassic style and consists essentially of a large circular hall or rotunda. On the front of the parapet of the gallery which surrounds the central hall are 16 plaques giving the most significant dates in

[1] E. C. Watson, Am. J. Physics 9, 184–185, 237–238, 307–309 (1941).

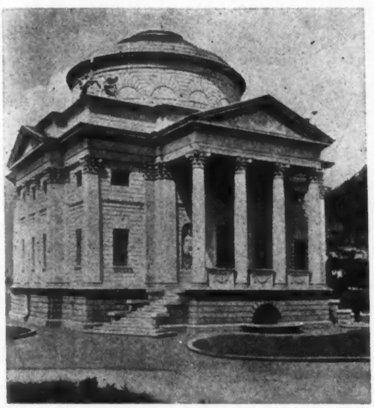

Plate 1. Exterior view of the Volta Temple in Como.

Plate 2. Interior view of the Volta Temple in Como.

Plate 3. Bas-relief representing Volta teaching at the University of Pavia.

Plate 4. Bas-relief representing Volta demonstrating his pile to Bonaparte at Paris in 1801.

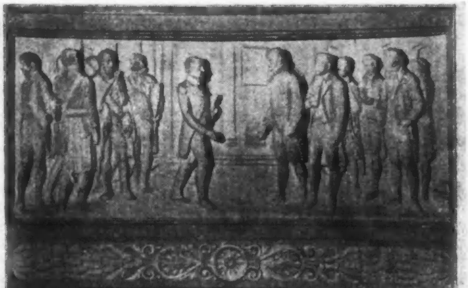

Plate 5. Bas-relief representing Volta receiving the Emperor Napoleon at Pavia in 1804.

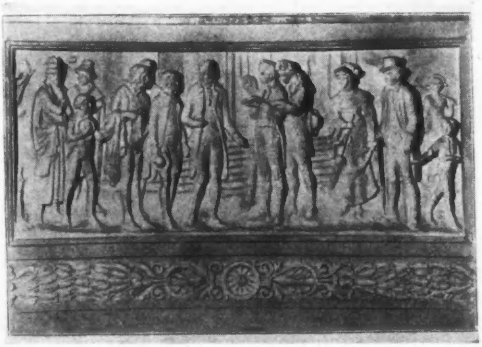

Plate 6. Bas-relief representing Volta prophesying telephonic communication as he leaves the church at Lazzate.

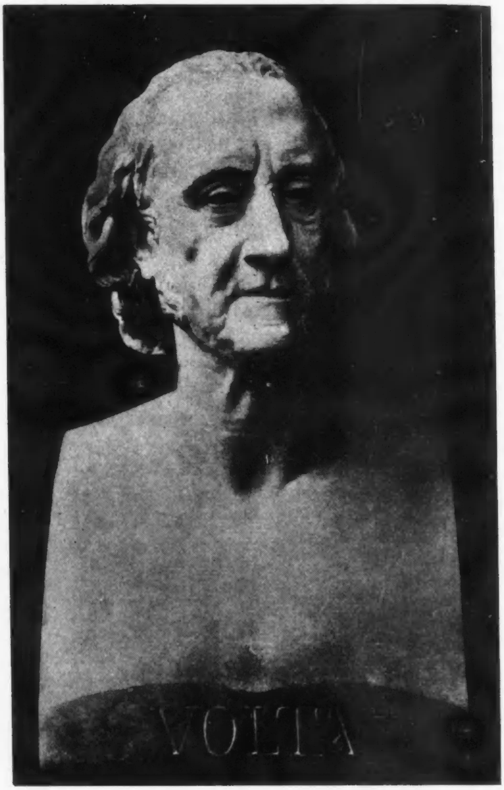


Plate 7. Bust of Alessandro Volta (1745–1827).

VOLTA's life and four bas-reliefs representing him teaching at the University of Pavia (Plate 3), demonstrating his pile to Bonaparte at Paris in 1801 (Plate 4), receiving the Emperor Napoleon in Pavia in 1804 (Plate 5) and prophesying telephonic communication while chatting with the country folk as he leaves the church at Lazzate where he used to pass his holidays (Plate 6). These bas-reliefs are the work of the sculptor PIETRO CLERICE. The building itself was designed by FEDERICO FIGERIO.

The central court contains a bust of VOLTA (Plate 7) on a tall column, and beneath the gallery are glazed cases in which are the fragmentary remains of the relics rescued from the disastrous fire of July 8, 1899, and exact reproductions of the original instruments. No pains were spared by the donor of this unique museum, FRANCESCO SOMAINI, and his coadjutors in making these reproductions as nearly like the original instruments as was humanly possible. Among the instruments invented by VOLTA and here exhibited are the electrophorus, the condenser, the electrostatic balance, very sensitive straw micro-electrometers and the first electrolytic cells.

In display cases located in the gallery are arranged VOLTA's records, manuscripts, medals, and many other varied and interesting mementos, together with the national edition of his works.

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*THE art of instructing and enlightening men . . . [is] the noblest portion and gift within human reach.*—D'ALEMBERT.

# NOTES AND DISCUSSION

## Centihg and Millihg

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THE two words, "centihg" and "millihg," pronounced as if written without the "h," were invented for a special purpose. They even have abbreviations, "cmhg" and "mmhg," respectively. It might reasonably be assumed that no further plea for their acceptance is necessary, but perhaps it should also be stated that they are units of pressure. The real question is whether any one else wants to make use of them for the sake of clearness and brevity.

As a matter of fact, it is perhaps more important that the abbreviations be used than the names themselves. If the word "millihg" grates harshly, and "millimeters of mercury" is more soothing, so be it; but why not replace the misleading "760 mm" by something less clumsy than the otherwise acceptable "760 mm-of-Hg"? After all, "760 mmhg" is rather obvious.

The other "ihg" units weren't invented, as there is practically no use for them—and there may be no use for these. Yet, if no more can be done, at least try "cmhg" and "mmhg."

## Another Demonstration of the Bernoulli Principle

F. E. KESTER
*The University of Kansas, Lawrence, Kansas*

IN the usual apparatus for demonstration of the Bernoulli principle, the cross-sectional area of the path of fluid flow is not uniform, thus producing changes in the velocity of the fluid. In the present apparatus, the cross-sectional area of flow is constant, the variability in flow velocity being obtained by allowing part of the fluid to escape from the main stream.

The idea for this new design came about as follows. Upon mounting a pair of emergency gas burners in the furnace of my residence, I was surprised to see that the gas-mixture pressure was higher at points farther from the gas-air mixer. Each burner is a 2-ft length of 1-in. water pipe drilled with about 50 holes of $\frac{3}{16}$-in. diameter uniformly spaced along its length. It is capped at one end and joined at the other end to the gas mixer by two 45° turns and two 6×1-in. nipples. The fifty-odd flames are fairly uniformly graded in height along the pipe, indicating two or three times more rapid escape of gas at the capped than at the mixing end.[1] Aside from the fact that the gas pressure is higher at points more remote from rather than close to the mixer, there is the slightly disconcerting result that most of the gas is burned in the least effective part of this particular furnace.

The explanation on the basis of the Bernoulli principle is apparent. Because gas escapes through the side holes, the flow velocity becomes progressively smaller and hence the pressure progressively increases. This is, no doubt, the major cause of the effect described. Just how much of the result may be attributed to the turbulence set up at the mixer (itself a Bernoulli principle device), and gradual dissipation of this kinetic energy into heat as the gas flows along the pipe, is difficult to see.

Because the actual burners were too useful in their assigned role to be removed for curious study, a model was made on simpler lines—a 30-in. length of 1-in. pipe with similar mixer at one end, cap at the other end, and with about fifty $\frac{3}{16}$-in. holes uniformly spaced in the last 24 in. Figure 1 shows the behavior of this model under three different conditions of operation.

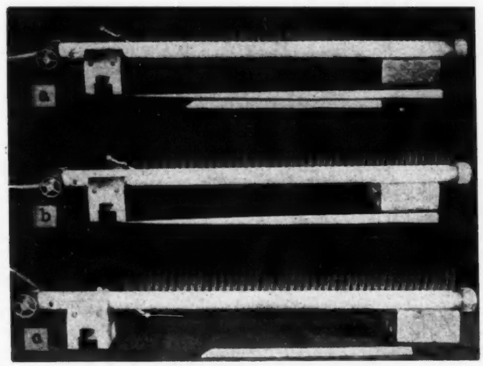

FIG. 1. (*a*) Normal behavior. (*b*) A 12-in. length of small quarter-round is laid in the cap end of the pipe, occupying about one-third of the sectional area. (*c*) A 2-ft length of 1-in. dowel rod, planed down to about seven-tenths of its diameter at the cap end and to practically a point at the other end, is laid in the active part of the burner.

The modification of the cross section of the burner shown in Fig. 1(*c*) is a result of a cut-and-try effort to obtain approximately uniform gas pressure along the length of the burner. Theoretically, the profile of the dowel rod that will give uniform velocity of flow along the active length of the burner has the equation

$$\pi r^2 x/L = y(r^2-y^2)^{\frac{1}{2}} + r^2 \sin^{-1} y/r;$$

here the $x$ axis is along the axis of the pipe, the origin of coordinates is at the middle point of the active length $L$, and the radius of the pipe is $r$. The equation involves the assumptions that the gas-air mixture escapes at the same rate from each of the holes and that the flow is nonturbulent and without friction. However, for more or less obvious reasons, no attempt was made to cut a dowel rod with such a profile.

As for the actual furnace burners, a more desirable distribution of gas combustion was obtained by enlarging the first 16 or 18 holes to $\frac{1}{4}$-in. diameter and by laying an 8-in. length of $\frac{3}{8}$-in. gas pipe in the far end of each burner.

[1] When water was substituted for the gas, the result was of the same type—jets higher at the capped end.

# DIGEST OF PERIODICAL LITERATURE

## Check List

**The inverting eyepiece and its evolution.** E. W. Taylor, *J. Sci. Inst.* **22**, 43–48 (1945). From a simple convex lens to the elaborate modern systems utilizing as many as seven component lenses. Constructional details of various types of eyepiece are given in a table.

**Certain applications of physical principles to the playing of musical instruments: the piano.** W. F. G. Swann, *J. Frank. Inst.* **239**, 163–184 (1945). Is a pianist able to modify the quality of tone produced in the case of single notes played with the same intensity?

**How illuminants are born.** S. G. Hibben, *J. Frank. Inst.* **239**, 391–401 (1945). Modern light sources and their characteristics.

**The coordination of the work of the physics, mathematics and electrical engineering staffs in the formulation of communications and electronics curricula, including ultrahigh-frequency techniques.** E. A. Guillemin, *J. Eng. Ed.* **35**, 401–406 (1945).

**Technology as action and science.** N. M. Oboukhoff, *J. Eng. Ed.* **35**, 472–477 (1945). The place of the science of technology among other sciences.

**A placement examination for mechanics.** W. E. Wilson, *J. Eng. Ed.* **35**, 537–545 (1945). An objective-type test for predicting the performance of students in the engineering mechanics course.

**Mobilization of scientific resources. V, the U. S. Army.** *J. App. Physics* **16**, 189–256 (1945). A series of articles on the organization of research in the U. S. Army.

**Physics in the medical program.** L. I. Bockstahler, *Scalpel of Alpha Epsilon Delta* **15**, 43–44 (1945).

**Mathematics for physicists.** H. T. H. Piaggi, *Nature* **154**, 355–356 (1944). A review of "The teaching of mathematics to physicists," a joint report of the Mathematical Association and the Institute of Physics (Spencer House, South Place, London, E. C. 2).

**Problems of modern physics.** J. Frenkel, *Nature* **154**, 417–421, 450–454 (1944). A survey of important unsolved problems, both experimental and theoretical.

**Fires of electrical origin.** Anon., *Nature* **154**, 574 (1944). In England, some 35 percent of all fires attributed to electrical origin arise from faults in the fixed installations. Of total fires, only about 2 percent are attributable to installation defects. Evidently more attention should be given to the quality and maintenance of *portable* domestic appliances and the flexible connections to them. The indications are that the total number of fires will substantially decrease as electricity supplants other fuels for heating and cooking.

## On the Passing of "Phys." from Our Literature References

IN footnotes and other bibliographic references to periodical literature, we will in future issues spell out "Physics" and "Physical" in the names of periodicals: thus, "Am. J. Physics," *not* "Am. J. Phys."; also "Rev. Mod. Physics," "Physical Rev.," "J. App. Physics," "J. Chem. Physics," "Ann. physique," "Ann. Physik," and so on. Except for these variations, we will continue to use the abbreviations given in *List of periodicals abstracted by Chemical Abstracts* (Columbus, Ohio, 1936, supplemented 1942). The abbreviations recommended in this useful publication—it lists some 3700 technical journals—have been adopted as standard by the International Union of Chemistry.

The term "physics," "physical" and "physicist" will bear frequent repetition, for they still are not widely known or understood outside of the sciences, or even within some of them. To an outsider, "phys." is as likely as not to mean "physiology" or "physiography," even though the standard abbreviations for the latter words are "physiol." and "physiog." Indeed, the American Antivivisection Society has recently distributed a leaflet in which the *American Journal of Physics*—the name spelled out completely—is credited with having published certain experiments allegedly involving torture of dogs. Actually, of course, the credit belongs to the *American Journal of Physiology*.

A certain philosopher, in a popular book with a special chapter on trends in physical science, asserts in all seriousness that physics "is rapidly becoming biological in character;" for, he queries, Eddington himself admitted in one of his books that *action* is today a dominant concept in physics, and is not action also a dominant characteristic of all things that are living? If he had read a little farther in Eddington, he would have found some really delectable supporting items: electron pairing, excited atoms, slow neutrons, fission, stripped atoms, unstable models, . . . . With a trend such as this in our terminology, even the change from "phys." to "physics" may not be enough to keep us out of trouble.—D. R.